Knowledge.

With which is incorporated Hardwicke's Science Gossip, and the Illustrated Scientific News.

A Monthly Record of Science.

Conducted by Wilfred Mark Webb, F.L.S., and E. S. Grew, M.A.

OCTOBER, 1912.

FURTHER REMARKS ON THE TRUE STRUCTURE OF THE DIATOM VALVE.

By T. F. SMITH.

(*Continued from page 356.*)

OUR knowledge of the secondary structure of diatoms may be said to date from a few years after the advent of the oil-immersion objective, more especially from the epoch-marking paper, already alluded to, read by Messrs. Nelson and Karop before the Quekett Club in 1886. The first oil immersions showed no improvement upon the water immersions, due to the splendid pitch to which these last had been brought in this country. In the interval, however, between 1878 and 1886, the N.A. of the oil-immersion had been worked up to 1·43 by Messrs. Powell and Lealand; Mr. Nelson became the owner of one, and hence the paper: "On the Finer Structure of certain Diatoms." In the introduction the authors say: "On examining certain Diatoms with the finest oil-immersion objectives, and under conditions of illumination such as are absolutely essential if the full aperture and, therefore, resolving power, of these glasses is to be utilised, some details of structure are brought into view which are otherwise quite invisible, and, so far as we know, have not hitherto been correctly described or properly figured. Acting on this belief, we have ventured to bring before your notice some short observations, accompanied by careful drawings, recently made on a few well-known forms."

FIGURE 404. *Coscinodiscus asteromphalus.* Objective used, Swift and Son's one inch of 0·30 N.A., orthochromatic plate, no screen, magnification three hundred and fifty diameters, exposure ten minutes.

This was but the first of a splendid series of observations, continued by Mr. E. M. Nelson right down to the present time. Others, of course, have followed, and as Dr. Dallinger said in the last edition of Carpenter (1901): "The nature of the delicate markings with

XUM

which almost every diatom frustule is beset has been one of the most interesting enquiries of the students of these forms since the introduction of the homogeneous, and especially the apochromatic, objectives; and it cannot be doubted that certain peculiarities of structure have been demonstrated which were never before seen." One is aware that this does not constitute the whole work of the microscope, or even the better part, yet it possesses this distinction that it has paved the way towards making other work possible. The President of the Royal Microscopical Society said on the reading of a recent paper on diatom structure, that: "The Society was always grateful for any work on diatoms, as this work had, more than any other, led to the perfecting of our microscopic lenses."

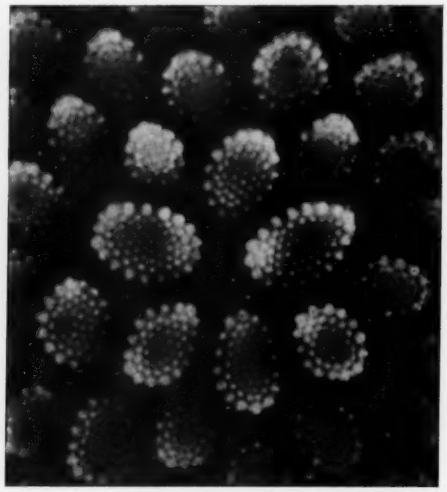


FIGURE 405. *Coscinodiscus asteromphalus*, centre, but from another valve. Objective used, Baker's oil immersion, one-twelfth inch of 1·30 N.A., ordinary plate, no screen, magnification two thousand seven hundred and fifty diameters, exposure twelve minutes.

Recurring to Messrs. Nelson and Karop's paper: occupying the centre of their plate is *Coscinodiscus asteromphalus*, of which it may certainly be said the finer structure had never been seen nor figured before. More, indeed, than *T. favus* the visibility of the finer details upon this form is due entirely to the oil-immersion objective, being beyond the compass of a dry lens. The general form and appearance of this diatom under an apochromatic twenty-four millimetre of Zeiss is given in Plate 1 of the Dallinger "Carpenter," and in the same plate a photomicrograph of a portion of the valve at two thousand diameters. Both are by Mr. E. M. Nelson. It may seem rash of the present writer to attempt to follow in the footsteps of so splendid a manipulator. Perhaps he would not, had it not been to prove a point—the identity of structure in all diatoms. This form also is well to the front, much discussed, just now, when it is as well to be in the fashion.

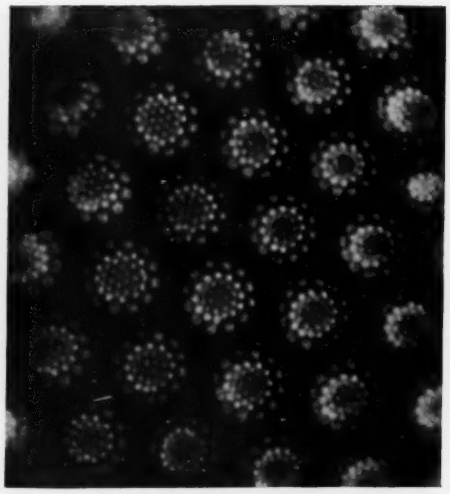


FIGURE 406. *Coscinodiscus asteromphalus*. From the side of the same valve as Figure 404. Objective, Baker's one-twelfth inch oil-immersion, ordinary plate, pot green screen, magnification two thousand seven hundred and fifty diameters, exposure three hours.

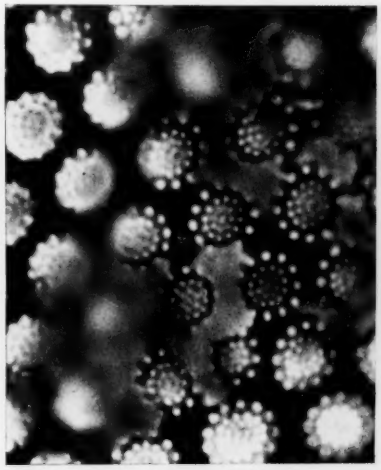
It will be seen that the rendering here differs from Mr. Nelson's, though it is in accordance with Dr. T. W. Butcher's, in his paper published in *The Journal of the Royal Microscopical Society* for December last, Mr. Nelson always went for the black dot as being the most truthful; the present writer prefers the white as being the most pretty. As for truth, he does not believe that there is a pin's difference to choose between the two.

Into the merits of the controversy between these authorities it is not proposed to enter here. The writer's own experience has been unfortunate in this matter. Wishing to find out for himself, and seeing a slide advertised as being prepared to show Mr. Nelson's tertiary structure, he bought one, only to be confronted with a vision of eye-spots. They were upon forms so small, too, that it would have been difficult to make out the secondary structure even had they been the right way up. His own knowledge, therefore, upon this point still remains in abeyance.

Figure 404, showing the whole disc, is taken by a one inch of Swift & Son, of 0·30 N.A., magnified three hundred and fifty diameters. Mr. Nelson's is under dark ground illumination, so in this case there are no points of comparison. Figures 405 and 406 show the normal aspect with white dot focus of parts of the valve under an oil-immersion, magnified two thousand seven hundred and fifty diameters. Dr. Butcher has so accustomed us to heroic magnifications that one is bound to follow his example, if only to compare results. Yet one remembers the time when one thousand diameters was looked upon as the efficient maximum. Yet the lenses were the same as now; further, nothing so far has been seen under the microscope which cannot be readily made out with this power. The white dot in Figure 405 must be taken with a qualification. It is really half white and half black dot, due to the whole of the field not being in quite the same plane. One other point besides it does show, however: how beautifully the objective was in adjustment under the tube-length employed.

The next two Figures, 407 and 408, present examples of torn structure in this diatom. We have

in this form the same structureless membrane, stretched upon the top to cover the sides of the hexagonal framework, as in *T. favus*. In Figure 407 it is seen spanning a gap, and half-way across one of the characteristic circles of white spots. Note, also, the outer sides of the rings where the membrane is torn away, and see how exactly they agree, both in this and in Figure 408, with similar structure shown in Figures 392, 393, 395, 396, and 397. Can anyone doubt, also, that in Figure 405 we see the same disposition of structure as in Figure 3?



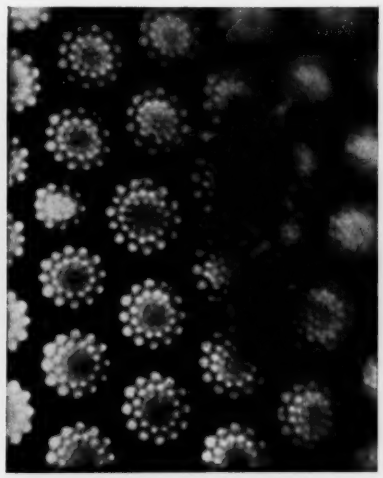


FIGURE 407. *Coscinodiscus asteromphalus*. Torn structure from another valve. Objective, Baker's oil-immersion, ordinary plate, no screen, magnification two thousand seven hundred and fifty diameters, exposure twelve minutes.

FIGURE 408. From the same valve as Figure 407.

In the torn structure of *T. favus*, the writer thinks, the compound triangular and discoid forms are constructed are the same. The hexagonal framework and the under layer of eye-spots can be demonstrated by direct proof — is analogy to stop short at this point because the finer structure at the top, though seen, cannot be so dealt with? When any detail of structure is no longer within the grasp of the aperture of the objective, it is only imperfectly resolved. Dr. Abbe, moreover, has told us in his paper, "On the relation of aperture to power," that under these conditions, all such details, whether triangular, square or lozenge shaped, assume the same rounded or oval outlines. In *P. formosum*, for

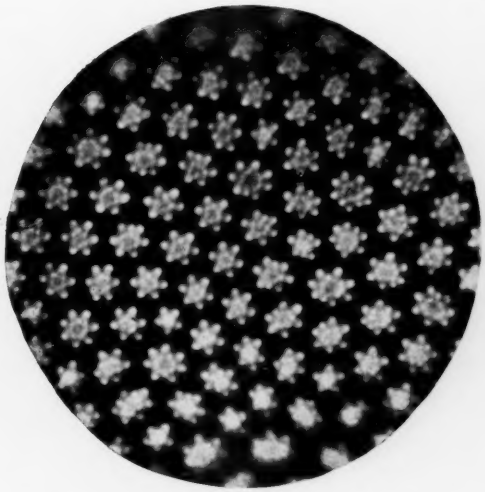


FIGURE 409. *Coscinodiscus centralis*. Objective, Baker's oil-immersion, ordinary plate, pot green screen, magnification two thousand seven hundred and fifty diameters, exposure three hours.

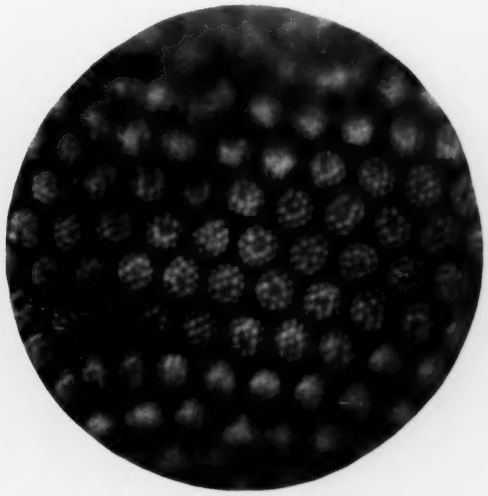


FIGURE 410. *Anlacodiscus Kittonii*. Objective, Swift & Son's one-twelfth inch oil immersion of 1·30 N.A., orthochromatic plate, yellow screen, magnification one thousand nine hundred diameters, exposure twenty minutes.

lies the key to secondary structure generally. One admits that when we come to the finer it must be more from analogy than from direct proof. If, on the other hand, we reject the inference we are met with this difficulty. The general lines upon which instance, the "heads" were always spoken of as round, yet under the 1·40 apochromatic in the writer's print of the broken valve with the postage-stamp fracture they come out distinctly square.

Figure 409 is another specimen of the discoid

form, *Coscinodiscus centralis*, where we seem to arrive at the region of imperfect resolution: that is, in the finer detail. The structureless covering over the hexagons is here arranged in a symmetrical pattern, reminding one of a tesselated pavement. Interspersed are the somewhat irregular patches of bright dots, well defined as to the outer ones, but running together and indistinct in the centres. An aperture of 1·40 would render them plainer, but even then some specimens defy resolution altogether.

Another and interesting form just on the borderland of resolution by an oil-immersion is *Aulacodiscus Kittonii*, a very difficult object to photograph, because of the inequalities of the surface, and a striking picture is impossible. There is a figure of this diatom in Plate 1 of Dallinger's "Carpenter," taken by Mr. Nelson with a low power. A drawing also of the finer structure accompanies a second communication to the Quekett Club by Messrs. Nelson and Karop, in 1887. There seem to be no published photographs of the finer structure, but an attempt is now made in Figure 410 to supply this deficiency, taken at one thousand nine hundred diameters. Only over a small part of the picture are the areolations in focus, and not good even at that. It should prove interesting, however, as presenting the general character of this form, though it cannot be produced upon the same plane. At the corners of the hexagons are stuck little bosses, slightly projecting, seen mostly at the right hand side of the print and at the top. Above all appears one of the processes (there are four in *A. Kittonii*) characteristic of this genus.

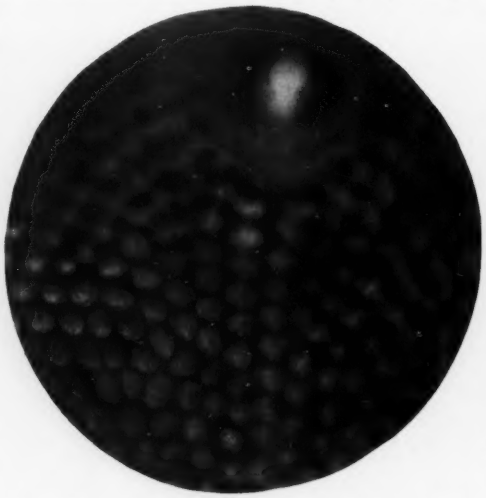


FIGURE 411. *Aulacodiscus Kittonii*, from another valve. Objective plate and screen the same, magnification two thousand seven hundred and fifty diameters, exposure half an hour.

Figure 411 is from the fragment of another valve, with smaller cellules but rather bolder areolations, taken at two thousand seven hundred and fifty diameters. No attempt is made here to include the bosses, though a few are indicated, as black spots, where the object is out of focus. At the top of the valve (not shown here) the surface is ruptured, producing a crack, and one of the broken edges shows the same zig-zag appearance, which may be said to be produced by a fibril. The evidence is positive which we could scarcely have expected to find amongst such fine structure. In another part of the valve the hexagons are torn away, leaving the rings of minute areolations projecting over the gap, as in Figures 407 and 408 of the much coarser details. One might, indeed, in some of these forms map out the spaces and produce an atlas. One might, indeed, go on to infinity producing examples, were it not that space, and the editors, forbid. The Figures 412–417 shew a number of fibrils taken from different diatoms of various size and contour.

FIGURE 412. Fibrils of *Pleurosigma formosum*, × 1750.

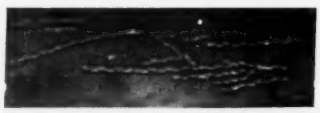


FIGURE 413. Fibrils of *P. halticum*, × 1750.

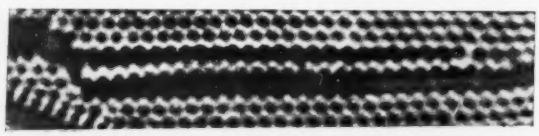


FIGURE 414. *P. angulatum*, × 3770.

FIGURE 415. Single fibril of *T. favus*, × 3000.

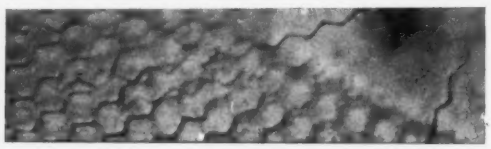


FIGURE 416. Fibrils of hexagons of small *Coscinodiscus* unravelling, × 3500.

FIGURE 417. Fibril of *Aulacodiscus Kittonii*, × 2750.

And here the writer thinks it is time to stop, saving his breath for a future occasion—perhaps?

ON STELLAR AND NEBULAR DISTANCES.

By PROFESSOR FRANK W. VERY.

Westwood Astrophysical Observatory, Westwood, Massachusetts, U.S.A.

(Continued from page 332.)

AMONG the possible modes of origin of a galaxy, we have the clashing of interpenetrating streams of matter moving in different directions. Within the region of intersection kinetic energy is more or less completely destroyed and converted into heat through collision and friction. Equal streams of meteorites moving in opposite directions are probably capable of producing in this way a fixed focus of most intense heat which perhaps might develop into a galaxy; or if the meteoritic swarms were unequal, or met at an acute angle, a resultant motion would continue in the composite aggregation. The first stars should be nearly devoid of local motion.

The dimensions of a galaxy may result in part from the size of the section of the original colliding meteor streams, and partly from the intensity of the perturbations (both astronomical and physical) which are engendered by the clash. Eventually, the meteoritic streams will come to an end, and the fiery rain of colliding particles will cease, making way for the interaction and further evolution of intensely heated bodies, born from the coalescence of the interacting masses. We have reason to believe that the Orion-type stars represent some of these early stages of stellar development.

It is now known that the galactic helium stars have not only small proper motions, but very small linear velocities in the line of sight. If we assume that they have developed recently from masses of matter in which kinetic energy has been largely annulled in the production of thermal energy, their slow motion is explained. Being too far apart and too uniformly distributed to acquire marked individual acceleration in definite directions, they may nevertheless continue to drift along in the residual direction which is proper to the swarm. Rancken, in 1882,* by considering proper motions of star-groups associated with the Milky Way, found evidence of a star-drift along the course of the stream. The research requires the segregation and separate investigation of special groups which are inextricably entangled and lost by indiscriminate methods of averaging. Rancken's research was confined to stars within 30° of the galactic plane, and the drift along the Milky Way appears to affect stars of the Sirian type principally; but these stars are only a little more advanced than stars of the Orion type. They move a little faster than the helium stars, and would barely show sensible proper motions at a distance of one hundred light-years. Like the Orion stars they might be suitable for disclosing the reflected solar motion.

In assuming an approximate diameter of one hundred and twenty light-years for the Galaxy, my purpose being to institute a comparison with the nebulae, it was expressly stated that only the brighter central regions were considered, for the reason that the less luminous parts are unlikely to be observed in very faint nebulae. It was to be presumed that the outer boundaries of the Galaxy, in its diffusely scattered borders, are much more distant than sixty light-years (in fact, I suggested for this outer limit a distance ten times as great); but regarding the brighter portions of the Milky Way as segments of slightly eccentric arcs of spirals along which myriads of stars are grouped in very much closer array than in our own portion of the great aggregation, the distance assigned to the nearest region of condensation is derived on the following suppositions: (1) That the novae, as a rule, are situated in the crowded spaces of the Milky Way; (2) that Nova Persei (1901) was one of these typical novae; (3) that it is not possible for matter to move with a greater velocity than that of light; whence it follows from the most rapid expansion of some parts of the nebulosity which appeared temporarily around this star, that its distance and that of the associated galactic stream, cannot be over sixty light-years.

There have been suggestions that the expansion of the nebulosity around Nova Persei may have been due to the motion of gaseous molecules driven off by light-pressure from the excessive intensity of luminosity at the maximum outburst of light; but in this case no one supposes that the velocity can have been at all comparable with that of light, and it follows that the star ought to be very near to us. This hypothesis, however, cannot be maintained, because measurements of the star's parallax reject the supposition. Professor Newcomb is the only astronomer of note who has contended that this marvellous occurrence bears witness to the existence in nature of velocities, in material particles, which are *greater* than that of light. By so doing, Newcomb opposed the unanimous opinion of physicists, basing his opposition, presumably, upon an exaggerated estimate of the accuracy of parallactic measures.

One other suggestion remains: which is, that the moving particles were positive ions electrically discharged. These may reach velocities of a high

* *Astronomische Nachrichten*, No. 2482.

XUM

order, even approaching that of light; and these discharges concerned particles whose masses were in the ratio 1 : 2 : 4, and thus explain a very remarkable feature of multiple emanations at these distances. For this reason it appears to me probable that this was the real nature of the phenomenon.*

When we recognize that stars which are at the same distance vary enormously in luminosity, we have to admit that, unless the object selected happens to be very near, the method of measuring relative parallaxes by comparing positions of neighbouring stars viewed from the ends of a radius of the Earth's orbit, and selecting faint comparison stars on the plea that they are probably farther away, is very unsatisfactory. The parallaxes of a few hundredths of a second obtained in this way are often fictitious. Even negative parallaxes may be found, showing that the faint stars were really the nearer. In spite of the great accuracy of instruments and observers, the means for discriminating between the objects selected remain insufficient. Hence it has seemed to me wiser to accept such an indication as has been furnished by the outburst of nebulosity around Nova Persei, rather than to trust to large numbers of apparent parallaxes of a few hundredths of a second, which are affected by the unknown parallaxes of the comparison stars.

Of twenty-eight stars between the fourth and tenth magnitudes, with annual proper motions ranging between 0″·01 and 3″·75, three which had proper motions of 0″·01, but which were among the brightest stars on the list (magnitudes 3·8 to 4·7), all gave to Dr. Schlesinger negative parallaxes large enough to show that they were more distant than the comparison stars.† The following are the mean parallaxes :—

4th magnitude	(2 stars)	... ...	$\pi = -0''{\cdot}002$
5th „	(4 stars)	... ...	$+0\ {\cdot}045$
8th „	(10 stars)	... ...	$+0\ {\cdot}085$
9th „	(8 stars)	... ...	$+0\ {\cdot}145$
10th „	(2 stars)	... ...	$+0\ {\cdot}173$

Most of these stars were selected on account of their large proper motions, but the four brightest ones were chosen to give examples of the Orion type. The series is interesting because it relates to fainter stars than are usually picked out for parallactic measurement, and because, like Eastman's list of proper motions cited above, it does not show any falling off with diminishing brightness. That a fourth-magnitude star of small proper motion may be farther away than a ninth-magnitude star of large proper motion, is a result which justifies the use of large proper motion, rather than that of brightness, as a criterion of nearness; but the phenomenon of star-drift suggests that proper motion, in turn, may fail as a test, especially for stars belonging to an assemblage which shares with our Sun in a common motion through space.

If the Sun were removed until it appeared as a seventh-magnitude star, it would still be three times as distant as the mean of the ninth-magnitude stars measured by Dr. Schlesinger. We are not making any large demand on probability if we assume that a particular strand of the Milky Way consists of seventh to fifteenth-magnitude stars no farther away than our hypothetical seventh-magnitude Sun, all moving in the same general direction and having neither recognizable parallax nor proper motion. If the Sun shares the motion only in part, a small stream-drift, such as is indicated by Rancken's investigation, remains.

An annulus has been assumed as a first approximation to the galactic shape by most investigators, because we have no means of differentiating between this shape and the more general two-branched nebular spiral, since the more remote turns of the spiral are superposed and projected on the same great circle of the sphere. The opposition of two narrower and brighter parts of the Galaxy, together with a similar opposition of the more widely diffused parts of the ring at points 180° apart, indicate that the Milky Way is really a two-branched spiral. If the Sun, instead of being in the central lumen of an annulus, is situated between two branches of a spiral, or as shown in Figure 359, the total breadth of the spiral figure may easily be five times the diameter of a simple ring, or the radius of the outer boundary of a circular disc may be five times the distance from the Sun to the nearer condensations; and some such unknown factor is implicitly understood in an estimate which purports to give only the order of magnitude involved. But if the linear dimensions are increased five-fold, the assumed luminosity of Nova Andromedae must be twenty-five times greater. This is perhaps not beyond the bounds of possibility; but to increase the distances fifty-fold, as Dr. Crommelin would have us do, increasing the brightness of the nova in the ratio 2,500 : 1, puts a strain on the probable luminosity of even such a remarkable object as a nova, because my estimate had already assigned to this star the status of one of the brightest of the novae. This dilemma was also instanced by Gore‡ as an argument against the ascription of so large a distance to the Andromeda nebula, and it has seemed to me conclusive.

If the Galaxy is a unit with two great branches which are the relics of the original movements, then two great opposing drifts, modified by minor local drifts, ought to surpass all others. The hypothesis hitherto accepted is thus stated by Newcomb §: (Supposition *a*) "There are scattered around us in the stellar spaces, in every direction from us, a large

* Frank W. Very, "An Inquiry into the cause of the Nebulosity around Nova Persei," *Am. Journal of Sci.* (4) Vol. XVI, page 49, July, 1903.

† Frank Schlesinger, "Photographic Determinations of Stellar Parallax made with the Yerkes Refractor." *Astrophysical Journal*, Vol. XXXIV, page 26, July, 1911.

‡ "KNOWLEDGE," July, 1909.

§ "The Stars," page 293.

number of stars, each moving onward in a straight line and in a direction which, with rare exceptions, has nothing in common with the motion of any other star."

Suppose that we substitute for this the following statement: (Supposition *b*) There exist scattered through space, in every direction, millions of stellar aggregations, each of which is a galaxy. Each galaxy is composed of many millions of stars moving for the most part in definite star-streams with velocities which increase with their age. As viewed from a given point within our Galaxy, the motions of the nearer stars may be grouped in a small number of prevalent drifts which may be under the gravitational control of great central masses, or highly-condensed clusters of stars, but the local drifts do not necessarily coincide with the main drifts of the dominant streams.

This supposition puts a very different interpretation on the facts of observation, and may reconcile difficulties which seem, at first sight, almost insuperable.

Newcomb gives in his work on "The Stars" a very accurate analysis of probabilities as applied to the nearer stars on supposition *a*. If we replace this criterion by supposition *b*, we have a less rigid rule, and some of the former conclusions are abrogated, or can be explained differently. For example, the percentage of negative apical motions, derived from the Bradley-Auwers stars, is found to increase as the cross-motion diminishes. Newcomb concluded that "this arises from the fact that in the case of the nearer stars the apical motions are necessarily larger, whether positive or negative." If, however, we suppose that not much beyond the general sphere of the nearer stars there are great star-streams moving nearly parallel to the Sun's way, there ought to be a large increase of negative apical motions on approaching the confines of the streams, together with a cessation of cross-motion.

The determination of the general motions of the Galaxy requires a knowledge of the trend of myriads of stars composing its major streams. The movements of a few stars, or even of a few thousand stars in several local clusters, or subordinate streams, are obviously inadequate to characterize the grand trend of the formation as a whole; but the greater the number of available stars the nearer we shall come to the ideal, especially if the phenomena of star-drift are studied. Professor Boss has made a slight move in this direction. In his first paper on "Precession and Solar Motion,"* he says of certain "closely accordant proper-motions: Each of these was condensed into one mean star representing the whole, except in the case of the moving cluster in Taurus, where four stars taken at random were employed as representative of the entire forty-one stars therein." To be entirely consistent, he should have classified the remaining stars by drifts, substituting for each drift one, or at most a very few mean representative stars. This he has not done, but has preferred the ordinary hypothesis that the peculiar motions of the stars are at random. In this way he has reached the conclusion that the mean motion of three thousand five hundred and forty-nine stars of sixth magnitude, or brighter, having annual proper motions less than $0''\cdot20$, is $\mu_\circ = 0''\cdot0566$, and that the parallactic motion at 90° from the solar apex is $M = 0''\cdot0399$; while, for five hundred and fifty-nine stars having annual motions between $0''\cdot17$ and $0''\cdot80$, $\mu_\circ = 0''\cdot319$ and $M = 0''\cdot2158$. That the ratio $\mu_\circ/M$ is nearly the same in either case testifies to a similar distribution of the stars of either selection among the several drifts, but does not disprove the existence of diverse drifts. Consequently, the variation of average proper motion in the two groups presumably signifies a real variation in the mean distances in the proportion of six to one, which is another testimony to the great extent of the local drifts.

When Professor Boss grouped his grand total of five thousand four hundred and thirteen stars according to their galactic latitude, a very different result was obtained. The ratio $\mu_\circ/M$ was found to vary as follows:—

Galactic latitude	−7° to +7°	+7° to +19° −7 ,, −19	+19° to +42° −19 ,, −42	> + 42° > − 42
$\mu_\circ/M$ =	1·18	1·25	1·36	1·62

Here we have a demonstration that the motions of the lucid stars are in some way associated with the direction of the galactic plane. It may be that most of the stars are moving in planes parallel to the galactic plane, and that the stars in low galactic latitudes have their motions foreshortened, as Professor Boss suggests; or it may turn out that the actual linear velocities diminish as the Galaxy is approached; but the main point is that the Galaxy is not so far away as to be out of touch with these stars.

The transformation of the proper motions into linear velocities will depend upon the value attached to measures of parallax, and thus upon the computer's point of view. The value of the mean motion for three thousand five hundred and forty-nine stars of mean magnitude 5·2 was $\mu_\circ = 0''\cdot0566$, and that for five thousand four hundred and thirteen stars of mean magnitude 5·7 was $\mu_\circ = 0''\cdot0538$, or a change of barely $0''\cdot003$ for a variation of half a magnitude. This, like other analyses of considerable numbers of stars, evidences only a slight connection between stellar brightness and proper motion.

One other piece of evidence will complete the sources from which we can draw our conclusion. It is known that the radial motions of the helium stars are exceptionally small. The fact was brought out by Frost and Adams,† and by Kapteyn and

* *Astronomical Journal*, No. 612, page 99, April 25th, 1910.

† "Radial Velocities of Twenty Stars." *Publications of the Yerkes Observatory*, Vol. II, pages 143 to 250.

XUM

Frost in their paper "On the Velocity of the Sun's Motion through Space as Derived from the Radial Velocity of Orion Stars."* Professor Boss contributed the further fact that their proper motions are also very small. Now, Mr. Benjamin Boss finds that several groups of Orion stars are moving in directions very little removed from position-angles which are 180° from the solar apex.† The proper motions for one hundred and sixty-two of these objects cluster around a mean value of 0″·0376. The apparent solar parallactic motion for the same areas (deduced from the entire body of stars included in the research of Professor Lewis Boss) is 0″·0349. Hence the motions in question are almost entirely parallactic. Here we have the very thing for which Dr. Crommelin asks, namely, a body of stars of galactic association (the mean galactic latitudes averaging + 11° and −12° on either side of the medial galactic circle), and one which reflects the solar motion. Moreover, this is the most convincing testimony that has yet appeared that the apparent solar motion is really solar. If the solar velocity is really as great as four astronomical units per annum, these stars and the associated Milky Way are 5·7 times as far away as the sixty light-years assumed. Further investigations, allowing for independent star-drift, may alter the value ascribed to the solar velocity, but the general order of the distances cannot be greatly changed.

Kapteyn and Frost (*Op. cit.*) find from sixty-one Orion stars having a mean cross-motion of 0″·0123 per annum, a radial velocity, freed from the parallactic velocity, of 6·3 kilometres per second, which corresponds to a parallax of 0″·00924, representing 5·4 times the galactic distance of my preliminary assumption.

There is an indication that the Orion-type stars are moving along two distinct galactic streams which have a trend oblique to the solar motion and away from some common centre, the radial drift for these stars near the solar apex being, in a general way, similar in direction to that of the Sun's motion, though less in amount, while on the opposite side of the heavens the radial drift is opposed to the Sun's motion. The result is that there is a difference of ten kilometres per second in the solar velocity derived from Orion stars near the apex and from Orion stars near the antapex. This conclusion is confirmed both by the measures of Kapteyn and Frost, and by those of Stroobant on Orion stars.

In view of the evidence which is presented here, it appears to me probable that my preliminary value of the galactic dimensions may need to be multiplied by five. The estimates of the nebular distances must also be increased in the same proportion, although it will, of course, be understood that, as this problem lies on the utmost verge of possible solution, any answer to it must be taken with considerable latitude.

[SUPPLEMENTARY NOTE.—Since writing the above, I have seen the article by Dr. Max Wolf in *Astronomische Nachrichten*, No. 4549, in which he also arrives at the conclusion that my earlier estimates of nebular distances and dimensions should be increased about five-fold, but basing his argument on the supposition that the parallax of Nova Persei is 0″·01, instead of 0″·05. This value seems inadmissible for the reasons cited here. The hypothesis that the Galaxy is a spiral with two branches and more than one turn, and that the Nova was on an adjacent branch, will remove this remaining discrepancy.]

* *Astrophysical Journal*, Vol. XXXII, page 83, July, 1910.

† "Systematic Proper-motions of Stars of Type B." *Astronomical Journal*. No. 620, page 163, December 5th, 1910.

EXTENSION LECTURES.

WE have pleasure in mentioning the work of the Extension Section of the Manchester Microscopical Society, which each year sends out a list of lectures of a popular character which can be given before societies in and about Manchester, the cost as a rule being limited to the expenses, as the work of lecturing and demonstrating is entirely voluntary and gratuitous on the part of the members. It is obvious, as a rule, that only a few shillings will be asked for from the societies which take advantage of the help which is offered. Occasionally a small fee is demanded, and this is the case where lectures are given before societies which are commercial undertakings, or are subsidised out of Government or public grants. The fifteenth list is before us for the session 1912–1913, and on it are sixty-eight subjects and the names of seventeen lecturers. We quote one or two of the titles:—"Some of Life's Simplest Children," "The Natural History of Lizards," "A Study of the Fertilisation of Flowers," "Household Pests," "Prehistoric Man," "The Management of an Aquarium." It will be seen that they are mostly natural history subjects, and secretaries of societies within reach of Manchester, will receive a copy of the list on application to Mr. R. Howarth, the Honorary Secretary and Treasurer, 90, George Street, Cheetham Hill, Manchester.

It would be a very good thing if other Societies were to follow in the footsteps of the Manchester Microscopical Society. The Council of the Selborne Society, we know, has arranged series of local lectures quite apart from its branches, and several of its members have given addresses to some of the local societies which are not able to pay large fees. The South-Eastern Union of Scientific Societies has for many years published lists of their members who are lecturers, with a selection of their subjects. This work, however, might be very well extended and systematised, and we should be glad to hear from any of our readers in any district who are willing to give lectures if their expenses are paid.

THUNDERCLAPS.

By REGINALD RYVES, F.R.MET.SOC.

WHILE the approach of a thunderstorm at night is heralded from a great distance by lightning without thunder, in the day time the growling of the distant thunder is often heard before the lightning is noticed, except by those who are in places where they can see for many miles in the direction of the approaching storm. As the storm comes nearer, the growling develops into a rumbling, but it may still be difficult to distinguish one peal from another, especially if there is a great deal of cloud in the sky. As soon as the peals are distinctly and separately heard we may begin to measure the distances and, if the storm approaches slowly and is well-defined and of small size, it may sometimes be possible to make a fair guess at the size of the storm by noting the distances of the furthest and of the nearest flashes. These distances are measured, roughly, by counting one mile to each four and a half seconds which elapses between the flash and the thunder. The first part of the peal is that due to the nearest part of the flash. When a big and very active storm is approaching the flashes are sometimes so frequent that incessant growling in the far distance is followed by incessant rumbling as the storm comes nearer, and even when the storm is quite near it may be impossible to distinguish one peal from another or even to tell whether the crash which follows several seconds after a flash of lightning is caused by that flash or by some earlier and more distant one. Storms of this character sometimes occur in the British Isles, and they not infrequently accompany severe cyclones in the tropics or mark the advance of the monsoons. These great currents, however, by no means always advance upon the countries which they traverse in the fashion which English writers love to describe; they often begin with ordinary showers or ordinary thunderstorms succeeding one another by longer or shorter intervals, and accompanied by moderate or even scanty rains.

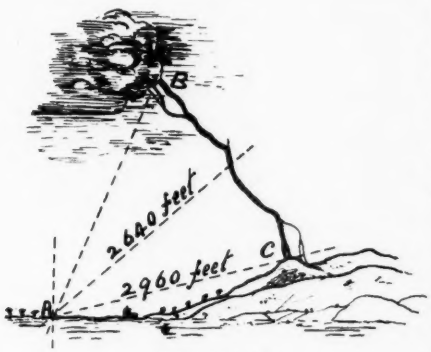


FIGURE 418.

Any very big circular disturbance, advancing with great masses of cloud and accompanied by smaller and more violent whirls, or areas of cyclonic type, may bring violent lightning and very heavy rain over a fairly broad belt of country; but very often the intensity of the storm depends upon the state of the atmosphere over the country to which it comes. The local condition of the atmosphere, the physical form or topography of the country and the small size of each satellite or subsidiary whirl, carried along with the main "depression" or cyclonic area, are the causes of the great differences in the local intensity of a storm, even when it is a big one. For instance, the great Derby Day storm burst with terrific force over Epsom Downs partly, no doubt, because one of the most intense of the minor whirls passed that way, partly because of the configuration of the country just there, and partly as the result of the electrical strain which already existed over the Downs. This condition of the atmosphere was shown for an hour or two before the storm came, by the occasional falling of very large drops of water, through the dry air, and from a sky only thinly clouded over at a great height. At Dorking there was moderate rain, in the Holmwoods hardly any, a little further south at Ockley the storm was very fierce accompanied by severe hail. Again, the terrific violence of the storm at Westerham on the main ridge of the country, may be compared with the relatively mild experiences in neighbouring places.

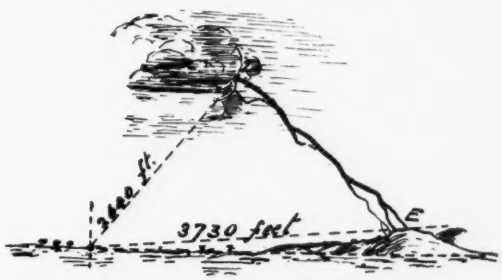


FIGURE 419.

MEASURING THE DISTANCE.

When a thunderstorm of an ordinary character has approached within a mile or so, or when it is actually passing overhead, it is usually possible to trace each peal of thunder to its flash and, by counting seconds between the flash and the first crack of thunder, to measure, at the rate of four and a half seconds per mile, the distance of the nearest part of the flash. It is a mistake, however, to suppose that by the loudness of the crash, and allowing for its distance, we can judge of the size or force of the flash of lightning. We are usually in error when we turn to one another and say "That was a tremendous flash" or "Something must have been struck." The

XUM

strength of the flash does, of course, make a difference; but there is something else which will seemingly make the noise of one flash many times louder than that of another which is quite as strong and is no further away. This is the direction taken by the flash relatively to the position of the observer.

Why Thunderclaps Vary.

Probably a good many of us have noticed that a very brilliant flash, quickly followed by thunder and therefore quite near, often causes only a moderately loud tearing noise, though a long and loud peal follows it. Soon after, perhaps, there comes a less vivid flash followed by an explosive crash or crack of great violence, though the peal which follows it is less loud than that which followed the gentler rending noise of the preceding flash. The explanation is very simple; the length and strength of the peal depend mainly upon the strength and size of the flash and only partly upon its position, while the loudness and sharpness of the crack, which comes before the peal, depends chiefly upon the direction taken by the flash. The crack, or rending noise, comes from the flash itself; the peal consists of echoes of this noise, coming from the clouds.

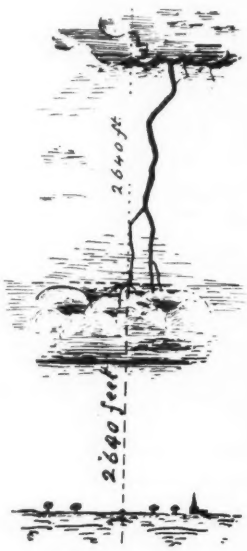


Figure 420.

Now, the noise of the actual flash comes to us, not from one spot, but from all along the path of the flash, and it is because of the length of this path that the time taken for the sound to come from the farther end of it is much greater than the time taken for the sound to come from the nearer end. It is only a matter of seconds, but it makes all the difference in the sharpness and loudness of the crack or rending noise. If the whole of this noise, from all along the path of the flash, reaches us in a quarter of a second, say, it sounds like a terrific thump or sudden crash; but if it takes two or three seconds for all of it to reach us, we hear it as a long rending noise. By noting with accuracy the duration of the rending noise of a big flash which could be identified by the exact time at which it occurred, three or four pairs of observers could measure the length of a flash of lightning and fix its position in the sky. The observers should be in pairs, and one of each pair would note the interval between the flash and the beginning of the crash, so as to get the distance, while the other, with a stop-watch, noted the duration of the crash. The possibility of doing this depends upon the two facts, that the time taken by the flash to traverse its path is immeasurably short, and that the time taken for the light of it to travel to the observer may also be counted as nothing.

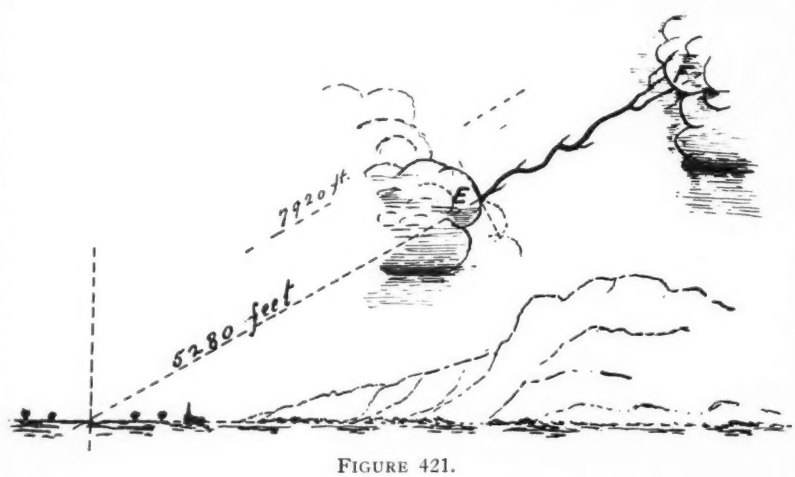


Figure 421.

Some Typical Examples.

The differences in the positions of flashes relative to the observer, and the effects on the duration of the crash or rending noise as distinct from the pealing echoes, are illustrated by the accompanying diagrams. If, for instance, the observer is at the point A in Figure 418, and a flash of lightning half a mile long and half a mile away from him passes from B in the cloud to C in another cloud or on the hill, the sound from the two ends of the flash will reach him in twenty-nine hundredths—less than one-third—of a second after that from the middle of the flash, since it only has to travel three hundred and twenty feet further. Sound travels through air at a speed of one thousand one hundred and forty-two feet per second. Suppose, now, that a flash half a mile long occurs as in Figure 419, passing from D to E, then the difference in distance between the longest path and the shortest path traversed by the sound is, in round figures, one thousand and ninety feet, and the noise from E will reach the observer nearly a second later than that from D. He will hear the noise of the crash spread out as a rending sound over the space of one second, and the noise will only be about a third as sharp as that of the former flash of the same strength.

Let us now look at Figure 420, which represents a flash half a mile long, passing between a cloud which is half a mile above the observer and another

cloud half a mile further overhead. In this case the sound of the flash will be spread over rather more than two and a quarter seconds. If both clouds were higher up, but the same distance apart, the "period of total arrival," as we may call it, would be the same, all the sound taking longer to travel the increased distances, all arriving later by the same interval of time. The flash would make a crash equally sharp but less loud because further away.

Turning now to Figures 421 and 422, if a flash starting from a point E, a mile away from the observer, strikes from cloud to cloud as in Figure 421, the noise from the far end reaches the observer about two and a quarter seconds after the noise from the nearer end. If, however, a flash which is a mile away at its nearest point strikes the cliff as in Figure 422, the whole sound of the crack reaches us in about one-seventh part of a second. We should, therefore, hear a crack about sixteen times as sharp as in the former case. Being also nearer it would for that reason be somewhat louder, but the effect of the new direction is, clearly, far more important. Apart from such differences as are produced by the positions of the clouds which are between us and parts of some of the flashes, we now see how it is that a very brilliant flash such as that in Figure 421 might be, may produce a noise much less sharp than that of a less vivid flash taking such a path as that in Figure 422.

It must not be supposed that the noise will be greater because the flash strikes the ground. When

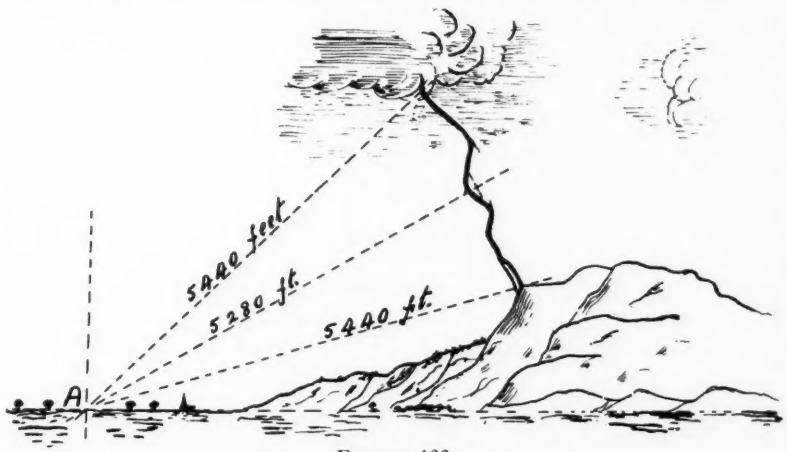


FIGURE 422.

electricity passes along, or is distributed in solids or liquids, nothing happens at all corresponding to what produces the crack or clap in the air; the expansion due to the heat of the flash and the sudden contraction following it. When lightning strikes into or near water the sound may reach the observer by way of the water more quickly than through the air, for sound travels more than four times as fast through water as it does through air; but on land the difference is not so great. Through solid granite sound

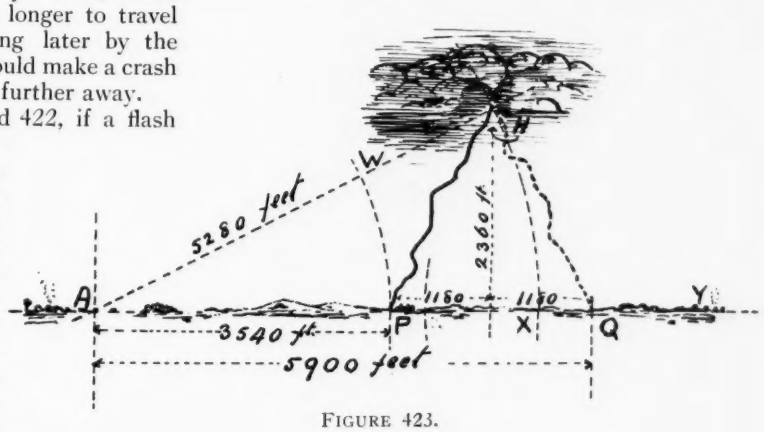


FIGURE 423.

travels half as fast again as through air; but since its speed through granite as it usually occurs in the hills is not much greater than the speed through air; and since, too, the rate at which it travels through wet sand is a good deal less than its speed through air, it is clear that as a rule the sound coming through the ground will lag behind that which comes to us directly from the flash.

A FLASH STRIKING THE GROUND.

When a flash strikes the ground the sharpness of the crash may depend upon where it strikes, and the nearer stroke is not necessarily the louder—strength and length being equal. Suppose that a flash starting from H in Figure 423, strikes the ground at Q, the difference between the nearest distance and the furthest distance is six hundred and twenty feet giving a "period of total arrival" of rather more than half a second. If a similar flash strikes at P, the nearest point is three thousand five hundred and forty feet away, the furthest five thousand two hundred and eighty feet, and the difference one thousand seven hundred and forty feet, giving a period of total arrival of more than one and a half seconds. In the former case the handicap is from Q to X, in the latter, from H to W. This figure, in which an arc of a circle with centre at A is drawn through H and X, suggests that some of the loudest crashes and those that are very loud considering their distance from the observer, are due to the lightning following a path which approximates to

the circumference of the arc, such as H X, so that all the noise reaches the observer in a small fraction of a second, a shorter period, even, than in such a case as that of Figure 418. Loudness is, of course, due in some cases to the flash being very near, say within a few hundred feet, when the nearness of the nearest part of it makes up for a necessarily fairly long "period of total arrival."

THE GOLD AND SILVER SHIELD.

In comparing accounts which different persons give of the same occurrence, the very diversity which is sometimes so informing to the expert merely serves to mystify those who have not studied the principles which dominate such occurrences. This is especially true of natural phenomena, with respect to which differences of position and of distance are not always accorded the consideration which they demand. Two witnesses may differ, and both be right. A very striking example of this is furnished by Figure 423. Leaving out of consideration the dotted flash from H to Q, let us fix our attention on the flash from H to P. Suppose that this flash struck a factory and did much damage, at exactly eight o'clock in the evening. At a quarter to eight, near the place marked Y, a man was, let us suppose, found murdered, just dying from a recent wound inflicted by a knife belonging to Tom Cornseed, a man employed on the farm at A. This man usually leaves the farm at about seven o'clock to walk to his home near Y. Some other circumstances point to his guilt, but two reliable witnesses swear that he was at the farm, A, *till the big flash of lightning that struck the factory.* These witnesses had no watches, and the usual work of the farm had been disturbed by the storm, which had also made the evening unusually dark. Their only knowledge of the time, with any accuracy, is that afforded by the great flash. This they describe as very bright, and followed by a loud peal, but not followed by a very sharp crack. A witness at Y, having an accurate watch, describes the stroke at eight o'clock as being followed by one of the sharpest and loudest cracks he ever heard. The two farm witnesses, re-examined, waver in their evidence, but under cross-examination go back to their first statement and hold to it doggedly. Is the man Tom Cornseed to be hanged? It depends upon whether anyone concerned is alive to the fact that the evidence is not conflicting, and is able to convince the judge and jury of this fact. What would a capable assessor do? He would first point out that the observer at Y, though the same distance from P as those at the farm A, might be, as in Figure 423, some one thousand feet or so nearer to the flash, on the average. He would then show, from Figure 423, how the point P might be only some four hundred and twenty feet further from Y than the point H, while the difference between the distance of P and H from A is one thousand seven hundred and forty feet. The "period of total arrival" at A is, therefore, more than four times that at the point Y, which accounts for the noise being less loud. So far the evidence is shown to be not necessarily in conflict with that by the witness at Y. If, now, the assessor can find witnesses from a mile or so away, to either side, or both sides, whose evidence shows that the flash did start as in Figure 423, then the evidence of the farm people is shown to be in accordance with probability and is all the more likely to be reliable.

THE POSER AND THE INTERPRETER.

It is a remarkable fact—probably due to the arrogance of a certain type of person who calls himself a "scientist"—that a very large number of persons set their faces resolutely against the acceptance of explanations of natural occurrences. The same persons will accept without question the statement that Mr. Edison has invented an electrical storage battery which will revolutionize all applications of power; or that one Tesla, has found out how to communicate, electrically of course, with the planets. They cannot believe, however, that an ordinary person who neither wears spectacles nor smokes strong cigars can possibly be able to explain how the tides occur, or why the lightning speaks sometimes with an explosive crack, sometimes with a rending crash, and sometimes with a loud peal which, repeated from many clouds, reaches when the distance is too great for us to hear the noise of the flash itself. Yet we make many mistakes because, in probing into the future, we fail correctly to interpret the present.

NOTICES.

EARLY MICROSCOPES. — In Hull Museum a serious attempt is being made to illustrate the growth and evolution of various exhibits, whether they be spinning wheels, bicycles, lighting appliances, or corsets, and an opportunity having recently occurred of showing a series of instruments bearing on the development of the microscope, Mr. T. Sheppard, the Curator, describes them in the August number of *The Naturalist*. The paper is illustrated, and the figures show specimens ranging from the "screw barrel" microscope held in the hand and used about the year 1725 to a more imposing one dated 1780. The latter is of interest because it belonged to Charles Sherborn, who lived from 1796 to 1858, to the late Charles William Sherborn, who died in 1912, and was presented to the Museum by Mr. Charles Davies Sherborn and his brother, Sidney Newton Sherborn.

WASPS REMOVING FROM THEIR NEST.—Mr. J. Edmund Clark, of Purley, sends the following interesting note to *The Gardeners' Chronicle* for August 31st. A powerful nest of wasps established itself among the Alpines last July in the garden of Mr. H. T. Mennell, Park Hill Rise, Croydon. One afternoon, as hundreds of the insects were flying in and out, Mr. Mennell and his daughter stood within two yards for some while, unmolested and unmolesting. They decided to treat the nest that night with boiling water and paraffin oil, for it was in level ground. Coming back a quarter of an hour later, they were astonished to find the wasps busier than ever, but all flying off in one direction over a fence, laden with eggs, grubs and pieces of the nest. In about an hour the place was practically deserted, as it has remained to date (August 22nd), though the hole has not been touched.

XUM

ON COOKED FOODS.

By KATHARINE I. WILLIAMS, B.Sc.

The University, Bristol.

IN this article the question of foods, especially cooked fish, vegetables and cereals is dealt with from the chemical point of view.

For many years the author has been engaged upon the investigation of the chemical composition of such food materials, and the suggestion has been made that the general public, and not only the chemical world, might be interested in this important subject. In America the question is brought before the public to a much greater extent than it is in England: a great deal of valuable information has been published there. Congress has granted money to the Department of Agriculture, bulletins on the subject are distributed freely, and several of the so-called Farmers' Bulletins deal with the uses of milk, fish, eggs and other ordinary kinds of nourishment.

It is hardly necessary to state that to be of real value all such investigations must be quantitative: that is, must show in percentages the amounts of the various constituents present. The first recorded quantitative analysis of any food was made in this country by George Pearson in 1795, who determined the proportion of water, starch, ash, fibre and extractive matter in kidney potatoes. For about eighty-five years similar work was carried on in Europe, chiefly in Germany. Since then many American chemists have devoted themselves to this subject. *Bulletin No. 28* (Office of Experiment Stations, Department of Agriculture), prepared by Professor Atwater and A. P. Bryant, entitled "The Chemical Composition of American Food Materials," contains details of the analyses of four thousand and sixty American articles and commodities used for human food in that country, but in most cases the food is uncooked.

During the last few years Professor Grindley, in the University of Illinois, and others have devoted themselves to the study of the effect of various methods of cooking meats.

It is well to understand what is meant by food, and the definition given by Dr. R. Hutchinson in "Food and the Principles of Dietetics" is perhaps the simplest that can be given—"anything which, when taken into the body, is capable of either repairing its waste or of furnishing it with material from which to produce heat or nervous and muscular work." Other substances may have a useful place in our dietary, though not falling into the category of food-stuffs. Later on it will be seen how various commodities answer to this description. The ordinary articles of diet are mixtures of various chemical substances, some of which are of value as nutrients, others are not. Of the former the most important are the nitrogenous; as the name implies they all contain nitrogen. They are known under various names: gluten in bread, legumin in the pulses, but are commonly spoken of as the proteins.

Non-nitrogenous is a general name for the carbohydrates, starch, sugar, and so on, also the fats, such as butter. Table salt and other mineral matters, and water, are also of importance; the latter is not only contained in the liquids we drink, but also forms part of the solids we eat.

The main object of the investigations now to be considered was to arrive at a clear idea of the value of foods as served at table, and the results show that many wrong impressions exist as to the proportion of nutrients they contain.

In commencing the study of a food, the first stage is to weigh the sample; then, if it is fish, it must be cleaned, the scales scraped off, the refuse weighed; with such vegetables as cabbage and broccoli the outer leaves must be removed; the pods of peas and beans all come under the head of refuse; the pods, however, have a value for soup. With potatoes the skins should not be removed before boiling, as valuable salts are mainly found in the layer just under the peel.

WASTE BEFORE COOKING.

In 100 parts.

Food.	Refuse.	Food.	Refuse.
John Dory ...	21½	Spinach	25
Gurnet	9	Green Artichokes	72⅓
Broccoli	68	Green Peas ...	45

The sample is again weighed, and cooked, and is then in the condition in which it would be served at table. It is then re-weighed to ascertain the increase or decrease in the amount of water gained or lost.

All waste is removed, such, for instance, as the bones and head of fish, the hard part of asparagus. With cereal foods, on the other hand, there is no refuse.

REFUSE AS SERVED AT TABLE.

Name.	Refuse.	Name.	Refuse.
John Dory ...	21	Haddock... ...	35
Salmon (section)	6	Asparagus ...	34
Herrings... ...	12	Green Artichokes	69

We now pass to the main question, what is the value of the edible portion of the various cooked foods? And the first point to consider is the amount of water, which is most important, as we could not live on dry foods; but, on the other hand,

XUM

it is clear that a high percentage of water will result in a bulky food. During the process of cooking, meat and fish decrease in weight; in almost every case vegetables increase, and cereals always do so.

Unfortunately, details were not kept as to the weight of the fish before cooking, but as will be shown, the percentage amount of water is lower in the cooked than in the uncooked samples.

With regard to meat it is stated that however cooked it loses from one-fifth to one-third of its weight.

Johnston found:—

	In Boiling.	In Baking.	In Roasting.
4-lbs. of Beef lose in weight ...	1-lb.	1-lb. 3-ozs.	1-lb. 5-ozs.
4-lbs. of Mutton lose in weight ...	14-ozs.	1-lb. 4-ozs.	1-lb. 6-ozs.

Professor Grindley observed that on boiling lean beef it lost forty-four and two-thirds per cent. of its original weight; at a temperature of 85° C., a similar sample boiled in boiling water lost forty-five per cent. He found, further, that beside losing water, some of the fat, mineral matters and protein are dissolved out at the same time, and on examining the extracts he found as the average of ninety-one samples that the loss of mineral matter was forty-four and a half per cent. of the total amount present in the meat, of fat twelve per cent., and of protein seven and a quarter per cent. In the process of stewing, highly flavoured soluble matters are removed from the meat and transferred to the bouillon.

At the same time comparing the nutrients in cooked and uncooked meats, we find a higher percentage of nutrients in the cooked condition and a lower percentage of water.

One great difficulty that confronts the food chemist is the material he works with: individually one sheep is not exactly like another, one may be fatter, another leaner; so with fish, vegetables and cereals. It is stated by Professor Snyder that in the case of flour containing twelve per cent. of moisture, if one hundred pounds be kept in a dry place a reduction of three pounds in weight may be observed, whereas in a damp place a corresponding increase may take place. Therefore, in this class of analysis only approximations can be made, and it is important to obtain as much information as possible, repeating the examination of articles of food, and taking the mean of a large number of analyses.

Thus, with three samples of uncooked American halibut it was found:—

Edible Portion.	Water.	Protein.	Fat.	Ash.
Minimum	70	17½	2·0	1·0
Maximum	79	20	11·0	1·1
Average	75	19	5·0	1·0

Six samples of mackerel were also analysed:—

Edible Portion.	Water.	Protein.	Fat.	Ash.
Minimum	64	17½	2	1
Maximum	79	19½	16	1½
Average	73	19	7	1

When first the work with vegetables was started it was impossible to judge how much vegetable was required: in three cases the supply fell short, and fresh samples had to be prepared the following year, giving very different results on the dry basis for protein.

	Sea Kale.	Parsnips.	Potatoes.
I.	41	12	13
II.	27	15	11

These data confirm the fact long known, that the nitrogenous compounds vary (*a*) with the maturity of the plant (*b*) variety of the plant (*c*) soil and cultivation (*d*) the season. In our present state of knowledge we assume all the nitrogen of our foods exists in the form of protein, but research is showing that some is present as non-proteid; further, that proteids differ considerably from each other: one form is found chiefly in nuts, another in meat. Often a mixture of various kinds is present, and the important question of the replacement of one form by another is being investigated by Dr. E. F. Armstrong and others. One important point requiring investigation is the question of loss in the cooking of vegetables. Whenever possible they should not be cooked in an excess of water, which it is necessary to drain off afterwards. Professor Snyder, at the University of Minnesota, made a study of the loss of nutrients in potatoes, cabbages and carrots; he states that one hundred pounds of uncooked cabbage only contain seven and a half pounds of solid matter, and of this two and a half to three pounds are lost in cooking, the loss consisting of protein, mineral matter, and carbohydrates. With carrots cooked in small pieces twenty per cent. to thirty per cent. of the total food material is extracted; the average of seventeen American analyses shows only 11·¾ of solid matter, while the average of thirty-five European analyses shows 13·2, and of this from two and a half to four pounds (mainly consisting of sugar, mineral matter and protein) is lost.

With potatoes it was found that the loss suffered on boiling was inconsiderable, provided they were unpeeled. With spinach we have only ten pounds of solid matter in one hundred pounds; when drained after cooking the loss is two and a quarter pounds. Celeriac nine and a-half pounds solid in one hundred pounds, and the average of three samples shows that four and a half pounds are lost. Borecole, or curly greens, one hundred pounds contain ten and one-third solid, and of this five and a quarter pounds are lost. Turning to a still more common article of food, rice, it is frequently boiled in an excess of water, which is then drained off. In this process protein, fat and

mineral matters are lost. Now rice contains but very little of these nutrients to start with, and in the East the native soldiers prefer to have the liquid which has been drained off, and leave the residue for the English soldiers. All this loss can be avoided by boiling the rice (according to the American receipt) in two and a half times its bulk of water for twenty minutes, then placing the saucepan on a tripod, covering it with a piece of cheese cloth, and allowing it to remain covered for an hour; at the end the rice will be tender and sweet. As a rule all vegetables, except in a very few cases, weigh more after cooking; all show a high percentage of water; so, in the case of loss of weight it must be due to the loss of nutrients. This increase of weight tends to make all vegetable and cereal foods bulky. One hundred ounces of green artichokes after cooking weigh three hundred and thirty-six ounces; one hundred ounces, brussels sprouts, one hundred and twenty-one ounces; leeks, one hundred ounces become two hundred and fifty-two ounces; one hundred ounces lentils, two hundred and thirty-eight ounces; one hundred ounces arrowroot, one thousand one hundred and fifteen ounces; one hundred ounces quaker oats, one thousand one hundred and ten ounces; one hundred ounces mother's oats, nine hundred and twenty-five ounces; one hundred ounces rice, four hundred and eighteen ounces.

The percentage of water is one of the most important points in food analysis; we want to know how much solid food we really consume. Considering meat and fish there is a higher percentage of solid matter, and the whole of the flesh consists of nutrients, fat, protein, and mineral matter. But in vegetable and cereal foods, on the other hand, besides these nutrients there is a framework of cellulose or woody fibre; according to some authorities this has a value from the food point of view, but it is doubtful. In the process of cooking the framework is ruptured, and the starch inside is gelatinised; the change may be observed in well-cooked potatoes.

Table Showing Percentage of Water and Solids in Various Articles of Food Before and After Cooking.

Name.	Cooked.		Uncooked.	
	Water.	Solids.	Water.	Solids.
Beef	57	43	71	29
Mutton (leg)... ...	51	49	63	37
Lamb	67	33	72	28
Veal (cutlets) ...	58	42	72	28
Cod	76	24	82	18
Haddock	68	32	78	22
Lentils	66	34	12	88
Green Peas	87	13	75	25
Dried Peas	62	38	14	86
Onions	99	1	82	18
Carrots	93	7	86	14
Cabbage	97	3	89	11
Vegetable Marrow ...	99	1	95	5
Rice	81	19	13	87
Quaker Oats ...	92	8	13	87
Arrowroot	93	7	16	84

The table shows the effect of cooking as regards the percentage of solid matter in the cooked and uncooked articles: increase in nutrients in the case of fish and meat, decrease in solids with vegetables and cereals following the cooking process. Only edible matter is considered: that is, the flesh of the meat and fish, and the portion of vegetables that can be eaten as food.

At this stage it is possible to judge which foods, bulk for bulk, contain most nutrients, but this does not conclude the matter; for it is further necessary to know the nature and the percentage of each nutrient. The human body requires certain amounts of fat, carbohydrates (that is, starch and sugar), mineral matter and protein daily. Protein can fulfil the functions of food in all respects as a tissue former, and also yields heat and energy, with the help of water and mineral matters; that is, salts such as sodium chloride or table salt; but under ordinary conditions fats and carbohydrates are necessary as energy-producers. Studies as to dietaries have been made in England, America, Germany, Sweden, Russia, and Japan among various classes of people, those doing hard and moderate work, factory operatives, tailors, college football teams, those employed in intellectual work, soldiers in time of peace and war. Taking the average, they conform fairly well to the standard drawn up by Professor Atwater: four and a half ounces protein, sixteen ounces carbohydrates, and four and a-half ounces fat for a man doing moderate work. To a certain extent fat and carbohydrates can replace each other, and it is estimated that for every part of protein four and three-quarters of carbohydrates and fat are required. It is understood that this standard is for the average man; in the case of women, ·8 of the above amounts is sufficient, and children require less in proportion to their age.

At the present time there is a good deal of discussion on the question of proteid, and experiments have been made by Professor Chittenden, of Yale, and others, who state that the human body only needs about half of the amount mentioned above, even two ounces are stated to be sufficient; but until more is known, Professor Atwater's or similar standards will hold their own.

Now the results of analysis can be stated in two ways, and one of these has led to the many false ideas which at present are in circulation among the general public. The dry powdered sample must be used for the estimation of fat, and so on, and in many books results are stated on this basis, and the important factor *viz.*: the percentage of water present is left out of the question. The other and correct method is to give tables including the water and calculating the fat, and so on, in the natural moist condition of the food as actually eaten. The two following tables will illustrate the false and correct method of describing the results of analysis.

XUM

Dry Powders.

(Analyses from which false deductions can be drawn.)

Names.	Mineral Matters.	Protein.	Fat.	Carbo-hydrates.	Fibre.
Oswego	1	23	—	76	—
Quaker Oats	3	22	4	83	1
Mother's Oats ...	2	12	4	85	1
Lentils	2	26	½	68	2
Peas	2	25	2	61	6
Brill	4	94	2	—	—
Halibut	4	80	16	—	—
Herrings	6	67	25	—	—
Beef (Boiled) ...	3	80	17	—	—
Veal (Roasted) ...	3	68	27	—	—
Mutton „ ...	2	51	46	—	—

Natural Moist Condition—as Served at Table.

(Correct Method of Analysis.)

Names.	Water.	Mineral Matters.	Protein.	Fat.	Carbo-hydrates.	Fibre.
Oswego ...	87	—	3	—	9	—
Quaker Oats...	92	¼	2	⅓	6	—
Mother's Oats	90	—	2	½	9	—
Lentils ...	66	⅔	9	—	23	⅔
Peas	62	⅔	9	⅓	23	2½
Brill	63	2	35	½	—	—
Halibut ...	74	1	20	4	—	—
Herrings ...	60	2	26	10	—	—
Beef (Boiled)	57	1	34	7	—	—
Veal (Roasted)	58	1	29	12	—	—
Mutton „ ...	51	1	25	23	—	—

One of the most important factors to consider is the amount of protein present in the food. In books treating of the subject the statement is often found that the pulses are rich in this nutrient, and for this reason are called "poor man's beef." Analysis clearly shows that such statements are entirely incorrect. But to understand why this view has been put forward it is only necessary to study the following table, which shows the composition of some of these foods in the uncooked condition, *i.e.*, not in the conditions in which they are eaten.

Approximate Composition of Uncooked Foods in the Natural Moist Condition.

Names.	Water.	Mineral Matters.	Protein.	Fat.	Carbohy-drates.	Fibre.
Beef	71	1	22	4	—	—
Veal	71½	1	20	6	—	—
Mutton ...	67	1	20	12	—	—
Lentils... ...	12	3	22	1	2	59
Peas (dried) ...	14	2	21	2	6	55

The samples used are the same as those analysed in the cooked condition, with the single exception of mutton. When these tables are compared it is seen that in the uncooked condition the protein of the sample analysed is nearly the same, but when served at table there is an enormous difference; the lentils and peas which before cooking contain twenty-two per cent. and twenty-one per cent. contain after cooking only nine per cent., and while beef containing twenty-two per cent. rises on cooking to thirty-four per cent., veal from twenty per cent., to twenty-nine per cent.; even on the basis of the cooked dry powder the amounts of protein present show that pulses cannot yield the same amount of protein as meat. The reason of this great difference is, of course, the change in the amount of water after cooking, a loss in the case of meat, and a large gain in the case of vegetable foods.

A new campaign has arisen lately whose watch-word is "Fish as Food." From what has been stated the amount of protein is satisfactory; but the difficulty is to get a really fresh supply, as when not fresh it is hardly a wholesome form of food, and to be avoided when served with various sauces to conceal the unsavoury smell in a stale condition. Many base their praise of fish on the idea that fish is an excellent brain food because it contains phosphorous, but Dr. Hutchinson, in "Food and the Principles of Dietetics," says: "It has never been shown that an increased supply of phosphorous in the food is specially favourable to mental effort, nor, indeed, has it been proved for any other food." But even if it were true that phosphorous is such an important point, a fish diet could be of no value; for the amount of that substance present is so extremely small.

As served at table the edible portion of herring only yields one-fiftieth per cent., sprats one-sixth per cent., trout one-seventh per cent., and turbot one-tenth per cent. in each case. Atwater made investigations on the same point on raw fish with a similar result.

The work of the food chemist is to analyse food materials, so that those more competent can draw up suitable dietaries; but even this is not an easy matter: to get a proper supply of the various nutrients needed, mixed rations are necessary. Lentils are excellent; but if we wish to obtain the full supply of protein from this source, it is necessary to consume one pound one and a half ounces per day (allowing ten per cent. for waste of protein in the process of absorption) and this when cooked would amount to four pounds six and a half ounces; one pound five ounces of cooked beef would serve the same purpose, and would be less bulky. The proteid from animal sources is said by most medical authorities to serve the body better as food than that from the vegetable kingdom; the fibre in the latter is also a disadvantage. Some of the everyday mixtures of food have a scientific value: bread and cheese, the former a source of carbohydrates, the latter for protein, bacon and beans, fat and protein, bacon with fowl to supply the required fat. No one food can supply all our requirements, except milk in the case of infants. We can obtain our supply of carbohydrates from two pounds thirteen ounces of bread, but to secure enough protein we should require three pounds fourteen ounces, giving a large excess of starch. With potatoes, eight pounds seven

XUM

ounces would supply the starch, but twenty-two pounds eight ounces would be needed for the protein, and this would be a very badly-balanced ration.

The money value of a food does not always agree with its true value in the ration: herrings are cheap and contain a high percentage of protein; properly cooked the cheaper joints of meat are as useful as the more expensive. The cheaper forms of arrow-root serve the same purpose as the most expensive. The body gets accustomed to certain foods. In America, for instance, it is said that immigrants from Southern Europe find it difficult to give up their maccaroni, olive oil, and their native kind of cheese, and take to the foods of their new home. Travellers in Switzerland will buy Brand and Bovril and complain of the price, while the Swiss-made Maggi answers the same purpose.

Professor Snyder, in "Human Foods and their Nutritive Value," calls attention to what he calls food notions—the false ideas that arise. Mushrooms are regarded as equal in value to beef, which chemical analysis entirely fails to confirm. Many valuable and wholesome foods are banished from the table, and incorrect views spread abroad. The value of a food must in the first instance be based on its chemical composition; after this the question passes into the hands of physicians and physiologists, and they alone can give the final verdict.

CORRESPONDENCE.

OBSERVATIONS OF SOME RECENT METEORS.

To the Editors of "KNOWLEDGE."

SIRS,—The following notes which I have made from observations of recent bright or otherwise unusual meteors may be of interest to some of your readers.

1912, May 5th.—At about $9^h\ 22^m$ C. S. T., a very brilliant and fine meteor was observed to pass just a small distance *s.* of δ Leonis, remaining visible for about two or three seconds, and moving in an E. to W. direction over about 8° of arc. During its flight it seemed to throw off matter from its head, thus leaving a train that was particularly bright immediately back of the head but that went out soon after the meteor was extinguished. The meteor was rich yellow in colour. Directly following this, a faint and rapid meteor was seen in practically the same part of the sky, while earlier in the evening one or two similar ones had been witnessed in the northern heavens.

1912, May 8th.—About $8^h\ 57^m$ a fairly bright, rapidly moving meteor was observed to pass from a point in the *s.* part of Ursa Major to a point approximately 7° *f.* α Leonis, or over a distance of roughly 35°. This object lasted during the time of about three seconds, and left no perceptible trail to speak of, but "went out" practically all at once, breaking very little in the action; just before its extinction the meteor apparently seemed to very slightly "swivel" in its course a bit toward the E., but whether this effect was merely an illusion, I do not know.

1912, May 17th.—At about $8^h\ 29^m$, approximately 10° *s.* β Herculis, I saw what was obviously a bright spark of light flash into view and instantaneously become extinguished, travelling, to my certain knowledge, an absolutely imperceptible distance, and leaving no visible train. The phenomenon could not have lasted more than $0{\cdot}5^s$. This was one of the most peculiar meteors it has ever been my fortune to observe.

1912, May 22nd.—About $8^h\ 30^m$ I observed an apparently faint, slowly moving meteor which became visible near Capella and travelled over about 5° of the sky *s.* of that star, before going out. It was partly obscured by deep haze or clouds, did not leave much train, if any, and, probably on account of the clouds, appeared to have a kind of bounding movement in the course of its flight, which caused it to be very noticeable.

1912, June 19th.—At approximately $13^h\ 45^m$ (C.S.T. astro.), I incidentally caught a fine meteor that moved from a point somewhere *p.* α Aquilae to a point possibly *f.* α Ophiuchi, (these uncertainties arise from that fact that I was unable to note accurately the positions of its start and finish), parallel to the plane of the horizon, and remaining visible probably as long as two seconds. It left no noticeable trail, was comparatively bright, and white in colour. After it disappeared, however, it left a beautiful and evident streak which remained apparent for some three seconds or more.

1912, July 23rd. While observing with my three-inch equatorial, at about $8^h\ 44^m$, I happened to glance up at the sky and saw a fine meteor move *n.* a few degrees, and, at its very best, become suddenly extinguished at a place approximately $1\frac{1}{2}$° or 2° *s.* and slightly *p.* β Aquilae. Just before the meteor went out it displayed in rapid succession several hues, but especially a light or pale blue. As it went out, it reminded me more of a great diamond sunburst in shape than any other thing I could think of, the rays of light from its nucleus apparently diverging shortly in all directions from the centre. This meteor was of comparatively small duration (being visible about $2{\cdot}5^s$ or 3^s), and was another one of the most peculiar examples of its class that I have ever witnessed, it seeming to suddenly go out while at its very brightest. Its colour changes were also interesting; but not as much so as its behaviour.

FREDERICK C. LEONARD,
President S.P.A., M.B.A.A.

ILLINOIS, U.S.A.

THUNDERSTORMS.

To the Editors of "KNOWLEDGE."

SIRS,—With reference to the statement of your correspondent, Mr. Tankerville Chamberlayne, that in reports of thunderstorms they are always described as coming up from a distance, and never as starting immediately overhead, it might interest him to know that out of the numerous thunderstorms we have had this year at Stafford, in two cases at least the first flash was overhead (provided a radius of one mile is reckoned as overhead).

In the first case, it was the bright afternoon of a showery day, when, without any warning (the sun was actually shining, or had been shortly before), there was a vivid flash of lightning, immediately followed by a loud peal of thunder; afterwards there was rain and some more thunder, but at a greater distance. This flash killed a boy, who was playing in a field with two other boys, about half a mile from where I was.

In the second case it was about 1.20 p.m., the sky was very overcast and there was heavy rain. I had just gone out of doors and had not taken many steps before there was a particularly vivid flash of lightning, followed by one of the most startling cracking peals of thunder I think I have ever heard. Afterwards there was more thunder and lightning, but at a much greater distance. This flash killed a cow in a shed situated about three-quarters of a mile from where I was walking.

In both these cases the first observed flash was practically overhead, and of course in the first case it was, as far as the boy that was killed was concerned, overhead in the strictest sense of the word.

In two other storms here this year two houses were struck within less than a mile from where I was, but in neither of these two cases have I any record as to whether they were struck by the first flash or not.

H. AUBREY P. HOWARD.

STAFFORD.

XUM

THE FACE OF THE SKY FOR NOVEMBER.

By A. C. D. CROMMELIN, B.A., D.Sc., F.R.A.S.

Date.	Sun. R.A.	Sun. Dec.	Moon. R.A.	Moon. Dec.	Mercury. R.A.	Mercury. Dec.	Venus. R.A.	Venus. Dec.	Jupiter. R.A.	Jupiter. Dec.	Saturn. R.A.	Saturn. Dec.	Uranus. R.A.	Uranus. Dec.	Neptune. R.A.	Neptune. Dec.
Greenwich Noon.	h. m.	°	h. m.	°	h. m.	°	h. m.	°	h. m.	°	h. m.	°	h. m.	°	h. m.	°
Nov. 1	14 25·3	S. 14·4	8 14·0	N. 25·0	15 30·9	S. 20·9	15 30·8	S. 23·0	17 0·1	S. 22·4	4 2·8	N. 18·5	20 8·2	S. 20·8	7 51·7	N. 20·5
,, 6	14 45·0	16·0	12 39·8	S. 4·2	16 0·5	22·9	16 57·2	24·0	17 4·5	22·5	4 1·3	18·4	20 8·6	20·8	7 51·6	20·5
,, 11	15 5·1	17·4	17 5·9	S. 27·6	16 29·3	24·3	17 23·9	24·7	17 9·0	22·6	3 59·7	18·3	20 9·2	20·7	7 51·5	20·5
,, 16	15 25·6	18·7	21 30·9	S. 19·1	16 56·3	25·3	17 50·8	25·1	17 13·7	22·7	3 58·0	18·2	20 9·8	20·7	7 51·3	20·5
,, 21	15 46·4	19·9	1 11·5	N. 8·4	17 19·3	25·6	18 17·7	25·2	17 18·4	22·8	3 56·3	18·2	20 10·5	20·7	7 51·1	20·5
,, 26	16 7·5	S. 20·9	5 50·2	N. 28·4	17 34·1	S. 25·2	18 44·6	S. 25·0	17 23·2	S. 22·9	3 54·7	N. 18·1	20 11·2	S. 20·6	7 50·8	N. 20·5

TABLE 41.

Date.	Sun. P	Sun. B	Sun. L	Moon. P	Jupiter. P	Jupiter. B	Jupiter. L_1	Jupiter. L_2	Jupiter. T_1	Jupiter. T_2	Saturn. P	Saturn. B
Greenwich Noon.	°	°	°	°	°	°	°	°	h m	h m	°	°
Nov. 1	+24·6	+4·2	90·0	+12·2	+ 5·5	−2·4	295·0	187·9	11 38 *e*	6 49 *m*	−3·0	−24·8
,, 6	23·6	3·7	24·1	+21·6	5·1	−2·4	3·3	213·0	11 55 *m*	3 55 *e*	2·9	24·7
,, 11	22·5	3·1	318·2	+ 4·8	4·6	2·4	71·5	248·1	0 12 *m*	3 5 *e*	2·9	24·7
,, 16	21·2	2·6	252·3	−17·5	+ 4·1	−2·4	139·3	278·2	6 1 *e*	4 20 *m*	2·8	24·6
,, 21	19·6	2·0	186·4	−20·8							2·8	24·6
,, 26	+17·9	+1·3	120·5	− 0·6							−2·7	−24·5

TABLE 42.

P is the position angle of the North end of the body's axis measured eastward from the North Point of the disc. B, L are the helio-(planeto-)graphical latitude and longitude of the centre of the disc. In the case of Jupiter L_1 refers to the equatorial zone, L_2 to the temperate zones. T_1 T_2 are the times of passage of the two zero meridians across the centre of the disc; to find intermediate passages apply multiples of $9^h\ 50\frac{1}{2}^m$, $9^h\ 55\frac{1}{2}^m$ respectively.

The letters *m*, *e* stand for morning, evening. The day is taken as beginning at midnight.

THE SUN moves South at a slackening rate. Sunrise during November changes from 6–53 to 7–44; sunset from 4–35 to 3–53. Its semi-diameter increases from 16′ 9″ to 16′ 15″.

MERCURY is well placed as an evening star, especially for Southern observers. On November 23rd the disc is half illuminated, semi-diameter $3\frac{1}{2}''$.

VENUS is an evening Star, drawing away from the Sun. Illumination five-sixths, semi-diameter $6\frac{1}{2}''$.

THE MOON.—Last Quarter $2^d\ 3^h\ 38^m m$; New $9^d\ 2^h\ 5^m m$; First Quarter $16^d\ 10^h\ 43^m e$; Full $24^d\ 4^h\ 12^m e$. Perigee $3^d\ 11^h m$, semi-diameter 16′ 11″; Apogee $16^d\ 10^h m$, semi-diameter 14′ 48″; Perigee $28^d\ 11^h m$, semi-diameter 16′ 16″. Maximum Librations, November 1^d 7° S., 10^d 5° W., 13^d 7° N., 22^d 5° E. The letters indicate the region of the Moon's limb brought into view by libration. E. W. are with reference to our sky, not as they would appear to an observer on the Moon.

MARS is an evening Star till 5th, but practically invisible.

JUPITER is an evening Star, increasing its distance from us, so that the equatorial semi-diameter diminishes from 16″ to $15\frac{3}{4}''$. The Polar is smaller by 1″. The configurations of the satellites at $5^h e$ are for an inverting telescope. (See Table 44.)

Date.	Star's Name.	Magnitudes.	Disappearance. Mean Time.	Disappearance. Angle from N. to E.	Reappearance. Mean Time.	Reappearance. Angle from N. to E.
1912.			h. m.		h. m.	
Nov. 1 ...	ω^1 Cancri	6·1	3 56 *m*	92°	5 9 *m*	300°
,, 1 ...	ω^2 Cancri	6·2	4 40 *m*	146	5 37 *m*	248
,, 16 ...	κ Capricorni	4·8	2 7 *e*	47	3 19 *e*	267
,, 18 ...	χ Aquarii	5·3	6 17 *e*	18	7 22 *e*	267
,, 24 ...	BAC 1055	6·9	1 25 *m*	4	—	—
,, 24 ...	36 Tauri	5·6	4 7 *e*	32	4 46 *e*	290
,, 25 ...	χ Tauri..	5·3	0 32 *m*	31	1 25 *m*	301
,, 26 ...	BAC 1746	6·5	5 7 *m*	95	6 7 *m*	271
,, 27 ...	49 Aurigae	5·1	2 10 *m*	67	3 14 *m*	305
,, 28 ...	*c* Geminorum	5·5	4 14*m*	127	5 20 *m*	269

TABLE 43. Occultations of stars by the Moon visible at Greenwich.

From New to Full disappearances occur at the Dark Limb, from Full to New reappearances.

Day.	West.	East.	Day.	West.	East.
Nov. 1	421 ○	3	Nov. 11	2 ○	134
,, 2	4 ○	13 2●	,, 12	2314 ○	
,, 3	14 ○	23	,, 13	34 ○	12
,, 4	2 ○	$1\frac{4}{5}$	,, 14	43 ○	2 1●
,, 5	$\frac{2}{5}$ 1 ○	4	,, 15	$42\frac{3}{4}$ ○	
,, 6	3 ⊙	24	,, 16	42 ○	13
,, 7	3 ○	124	,, 17	41 ○	23
,, 8	21 ○	4 3●	,, 18	42 ○	13
,, 9	2 ○	134	,, 19	4213 ○	
,, 10	1 ○	234	,, 20	34 ○	$\frac{3}{4}$

After November 20th, Jupiter is too near the Sun for convenient observation of satellite phenomena.

TABLE 44.

Satellite phenomena visible at Greenwich, $1^d\ 4^h\ 46^m\ 42^s$ III. Ec. R.; $6^d\ 4^h\ 35^m$ I. Tr. I.; $5^h\ 17^m$ I. Sh. I.; $7^d\ 4^h\ 37^m\ 35^s$ I. Ec. R.; $9^d\ 5^h\ 3^m$ II. Oc. D.; $18^d\ 4^h\ 48^m$ II. Tr. E.

All the above are in the evening hours.

The eclipse reappearances of I. II. and both phases of those of III. occur high right of the inverted image, taking the direction of the belts as horizontal.

SATURN is in opposition on 23rd. Polar semi-diameter $9\frac{1}{2}''$. The major axis of the ring is $47\frac{1}{2}''$, the minor axis $19\frac{3}{4}''$. The ring is now approaching its maximum opening and projects beyond the poles of the planet.

East elongations of Tethys (every fourth given). November $1^d\ 6^h{\cdot}9\ e$, $9^d\ 8^h{\cdot}0\ m$, $16^d\ 9^h{\cdot}2\ e$, $24^d\ 10^h{\cdot}3\ m$. Dione (every third given). November $1^d\ 1^h{\cdot}5\ e$, $9^d\ 6^h{\cdot}5\ e$, $17^d\ 11^h{\cdot}4\ e$, $26^d\ 4^h{\cdot}4\ m$.

Rhea (every second given). November $3^d\ 7^h{\cdot}6\ m$, $12^d\ 8^h{\cdot}3\ m$, $21^d\ 8^h{\cdot}9\ m$, $30^d\ 9^h{\cdot}6\ m$.

For Titan and Iapetus, E. W. mean East and West elongations, I., S. Inferior and Superior Conjunction, Inferior being to the North, superior to the South. Titan, $4^d\ 1^h{\cdot}4\ m$ W., $7^d\ 11^h{\cdot}7\ e$ S., $12^d\ 2^h{\cdot}7\ m$ E., $16^d\ 3^h{\cdot}1\ m$ I., $19^d\ 11^h{\cdot}0\ e$ W., $23^d\ 9^h{\cdot}2\ e$ S., $27^d\ 12^h{\cdot}0\ e$ E. Iapetus $12^d\ 3^h{\cdot}6\ e$ E.

URANUS is an evening Star, semi-diameter 2". It is $7\frac{1}{2}^c$ South of Alpha Capricorni, 5° South-West of Beta.

NEPTUNE is a morning star, not yet very well placed.

METEOR SHOWERS (from Mr. Denning's List):—

Date.	Radiant. R.A.	Radiant. Dec.	Remarks.
Nov. 1 ...	43°	+ 22°	Slow, bright.
,, 2 ...	58	+ 9	Slow, bright.
,, 10—12	133	+ 31	Very swift, streaks.
,, 14—16	150	+ 22	Leonids, swift, streaks.
,, 16 28	154	+ 41	Swift, streaks.
,, 20—23	63	+ 23	Slow, bright.
,, 17—23	25	+ 43	Andromedids, very slow,
,, 25 to Dec. 12	189	+ 73	Rather swift. [trains.
,, 30 ...	190	+ 58	Swift, streaks.

CLUSTERS AND NEBULAE.

Name.	R.A.	Dec.	Remarks.
♅ H. II. 224	$1^h\ 4^m$	N 35°·2	Faint nebula.
M. 33	1 28	N 30 ·2	Large faint nebula.
M. 76	1 37	N 51 ·1	Double spiral nebula.
♅ H. VI. 31	1 40	N 60 ·7	Cluster.
♅ H. VI. 33, 34	2 13	N 56 ·7	Fine double cluster.
H. 227	2 27	N 57 ·2	Cluster.
♅ H. I. 156	2 35	N 38 ·7	Bright nebula.
M. 34	2 37	N 42 ·4	Cluster.

DOUBLE STARS.—The limits of R.A. are 1^h to 3^h.

Star.	Right Ascension. h. m.	Declination.	Magnitudes.	Angle. N. to E.	Distance.	Colours, etc.
ξ Piscium	1 9	N 7°·1	4, 5	64°	23"	Yellow.
42 Ceti	1 15	S 1 ·0	6, 7	358	$1\frac{1}{2}$	White, bluish.
ψ Cassiopeiae	1 20	N 67 ·7	4, 8	110	25	Yellow, bluish.
Each Star has a faint companion distant 3".						
Polaris	1 25	N 88 ·8	2, 9	217	18	Yellow, white.
Lalande 3137	1 37	S 11 ·7	5, 7	87	4	White, bluish.
Piazzi I. 179	1 45	N 21 ·9	6, 7	164	3	Gold, blue.
γ Arietis	1 49	N 18 ·9	4, 4	359	8	Yellowish, bluish.
Piazzi I. 209	1 51	N 1 ·4	7, 7	36	$\frac{3}{4}$	White, period 136 years.
α Piscium	1 57	N 2 ·4	3, 5	318	3	Yellowish, greenish.
γ Andromedae	1 58	N 41 ·9	2, 5	62	10	Orange, emerald.
The companion is a close double, period 55 years.						
ι Trianguli	2 7	N 29 ·9	5, 6	75	4	Yellow, blue.
ι Cassiopeiae	2 22	N 67 ·0	4, 7	243	2	Yellow, blue.
There is also an 8th magnitude star, distant 7".						
ν Ceti	2 31	N 5 ·2	5, 9	83	8	Yellow, ash.
84 Ceti	2 37	S 1 ·0	6, 9	318	4	White, purple.
γ Ceti	2 39	N 2 ·9	3, 7	291	3	Yellow, blue.
π Arietis	2 44	N 17 ·2	5, 8	121	3	Yellowish, orange.
Piazzi II. 220	2 55	N 52 ·0	5, 7	85	12	Bluish, greenish.
ε Arietis	2 54	N 21 ·0	5, 6	202	1	White.

TABLE 45.

XUM

NOTES.

ASTRONOMY.

By A. C. D. Crommelin, B.A., D.Sc., F.R.A.S.

TABLES OF JUPITER'S SATELLITES.—There was an unanswered query in "Knowledge" some months ago, inquiring how the configuration of Jupiter's satellites could be obtained for other times than those given in *The Nautical Almanac*. If one neglects the mutual perturbation of the satellites, the problem is not a difficult one. Some simple tables have appeared in the *Boletin de la Sociedad Astronomica de Mexico* for June and July last, by Señor Enzo Mora. I think that the portion of these tables that is required for giving the *abscissa* of each satellite (*i.e.*, the apparent distance east or west of Jupiter's centre in terms of the planet's equatorial radius) will be of interest to a sufficient number of our readers to justify its reproduction here. The time for which the positions are required is to be expressed in Greenwich Mean Time (Day commencing at noon, as is the usage in astronomical reckoning). Then the six quantities, I, II, III, IV, m, n, are to be found by adding the quantities from the tables A_1, A_2, A_3, A_4, A_5, with arguments century, year of century, month, day of month, hour.

Table A_1.

Century Figure.	I.	II.	III.	IV.	m.	n.
16 (1600 to 1699) ...	35	203	287	775	303	711
17 (1700 to 1799) ...	690	801	857	850	869	141
18 (1800 to 1899) ...	345	401	429	925	434	571
19 (1900 to 1999) ...	0	0	0	0	0	0
20 (2000 to 2099) ...	221	881	711	135	569	429
21 (2100 to 2199) ...	876	481	283	211	135	858

Table A_2.

Year of Century.	I.	II.	III.	IV.	m.	n.
00	104	973	658	54	755	640
01	335	672	590	840	670	724
02	566	370	523	626	585	809
03	797	69	455	413	501	893
04	593	49	527	259	418	977
05	823	747	459	45	333	62
06	54	446	391	832	248	146
07	285	144	323	618	163	230
08	81	124	395	464	81	315
09	312	822	327	251	996	399
10	543	521	259	37	911	483
11	774	219	192	824	826	567
12	570	199	263	670	744	652
13	801	897	196	456	659	736
14	32	596	128	243	574	820
15	263	294	60	29	489	905
16	59	274	132	875	406	989
17	290	973	64	661	321	73
18	521	671	996	448	236	157
19	752	370	928	234	151	242
20	548	349	0	80	69	326
21	779	48	932	867	984	410
22	10	746	865	653	899	495
23	241	445	797	440	814	579
24	37	425	869	286	732	663
25	267	123	801	72	647	748
26	498	822	733	859	562	832
27	729	520	665	645	477	916
28	525	500	737	491	394	1
29	756	198	669	278	309	85
30	987	897	602	64	225	169
31	218	595	534	850	140	253
32	14	575	606	697	57	338
33	245	274	538	483	972	422
34	476	972	470	269	887	506
35	707	670	402	56	802	591
36	503	650	474	902	720	675
37	734	349	406	688	635	759
38	966	47	339	475	551	844
39	197	746	271	262	467	928
40	993	726	343	108	384	12
41	223	424	275	894	299	97
42	454	123	207	681	214	181
43	685	821	139	467	129	265
44	481	801	211	313	47	350
45	712	499	143	100	962	434
46	943	198	75	886	877	518
47	174	896	8	673	792	602
48	970	876	79	519	710	687
49	201	574	12	305	625	771
50	432	273	944	92	540	855
51	663	971	876	878	455	940
52	459	951	948	724	372	24
53	690	650	880	510	287	108
54	921	348	812	297	202	192
55	152	47	744	83	117	277
56	948	26	816	929	35	361
57	179	725	748	716	950	445
58	410	423	681	502	865	530
59	641	122	613	289	780	614
60	437	102	685	135	698	698
61	667	800	617	921	613	783
62	898	499	549	708	528	867
63	129	197	481	494	443	951
64	925	177	553	340	360	36
65	156	875	485	127	275	120
66	387	574	418	913	191	204
67	618	272	350	699	106	287
68	414	252	422	546	23	373
69	645	951	354	332	938	457
70	876	649	286	118	853	541
71	107	347	218	905	768	626
72	903	327	290	751	686	710
73	134	26	222	537	601	794
74	366	724	155	324	517	879
75	597	423	87	111	433	963
76	393	403	159	957	350	47
77	623	101	91	743	265	132
78	854	800	23	530	180	216
79	85	498	955	316	95	300
80	881	478	27	162	13	385
81	112	176	959	949	928	469
82	343	875	891	735	843	553
83	574	573	824	522	758	637
84	370	553	895	368	676	722
85	601	251	828	154	591	806
86	832	950	760	941	506	890
87	63	648	692	727	421	975
88	859	628	764	573	338	59
89	90	327	696	359	253	143
90	321	25	628	146	168	227
91	552	724	560	932	83	312
92	348	703	632	778	1	396
93	579	402	564	565	916	480
94	810	100	497	351	831	565
95	41	799	429	138	746	649
96	837	779	501	984	664	733
97	67	477	433	770	579	818
98	298	176	365	557	494	902
99	529	874	297	343	409	986

TABLE A_3.

Month.	I.	II.	III.	IV.	m.	n.
Jan., common year ...	664	399	767	478	848	986
Jan., leap year	99	118	628	419	846	986
Feb., common year ...	180	122	93	329	928	994
Feb., leap year	615	840	953	269	926	993
March ...	0	0	0	0	0	0
April	516	722	326	850	78	7
May	466	163	512	641	152	14
June	981	886	838	491	227	21
July	932	327	24	282	300	28
August ...	447	49	350	132	375	35
September ...	963	771	675	983	451	42
October ...	913	212	862	773	526	49
November ...	429	935	187	624	605	57
December ...	379	376	374	414	683	63

TABLE A_4.

Day of Month.	I.	II.	III.	IV.	m.	n.
1	0	0	0	0	0	0
2	565	281	140	60	3	0
3	130	563	279	119	5	0
4	695	844	419	179	8	1
5	260	125	558	239	10	1
6	825	407	698	298	13	1
7	390	688	837	358	15	1
8	955	970	977	418	18	2
9	520	251	116	478	20	2
10	85	532	256	537	23	2
11	650	814	395	597	25	2
12	215	95	535	657	28	3
13	780	376	674	716	30	3
14	345	658	814	776	33	3
15	910	939	954	836	35	3
16	475	220	93	895	38	3
17	40	502	233	955	40	4
18	605	783	372	15	43	4
19	170	65	512	74	45	4
20	735	346	651	134	48	4
21	300	627	791	194	50	5
22	865	909	930	253	53	5
23	430	190	70	313	55	5
24	995	471	209	373	58	5
25	560	753	349	433	60	6
26	125	34	489	492	63	6
27	690	316	628	552	65	6
28	255	597	768	612	68	6
29	820	878	907	671	70	6
30	385	160	47	731	73	7
31	950	441	186	791	75	7

TABLE A_5.

Hour of Greenwich Mean Time, Starting at Noon.	I.	II.	III.	IV.	m.	n.
0^h	0	0	0	0	0	0
1	24	12	6	2	0	0
2	47	23	12	5	0	0
3	71	35	17	7	0	0
4	94	47	23	10	0	0
5	118	59	29	12	1	0
6	141	70	35	15	1	0
7	165	82	41	17	1	0
8	188	94	47	20	1	0
9	212	106	52	22	1	0
10	235	117	58	25	1	0
11	259	129	64	27	1	0
12	283	141	70	30	1	0
13	306	152	76	32	1	0
14	330	164	81	35	1	0
15	353	176	87	37	1	0
16	377	188	93	40	2	0
17	400	199	99	42	2	0
18	424	211	105	45	2	0
19	447	223	110	47	2	0
20	471	234	116	50	2	0
21	494	246	122	52	2	0
22	518	258	128	55	2	0
23	541	270	134	57	2	0
24	565	281	140	60	3	0

The sums of the quantities from the above five tables are to be taken rejecting in the sum any integral number of thousands. Then apply to each of the sums I, II, III, IV, m the correction given in Table B, the argument being the sum of n just found; afterwards apply to the corrected sums I, II, III, IV the second correction given in Table C, the argument being the corrected sum of m. The second correction is

\+ when m is less than 500.
− „ „ „ greater „ „

TABLE B.

n.	Correction.	n.	Correction.	n.	Correction.
0	15	350	3	700	30
50	10	400	7	750	31
100	6	450	11	800	30
150	2	500	15	850	28
200	0	550	20	900	25
250	0	600	24	950	20
300	1	650	27	1000	15

TABLE C.

m.	Correction.	m.	m.	Correction.	m.
0	0	1000	260	30	740
20	+ 5 −	980	280	29	720
40	9	960	300	27	700
60	14	940	320	25	680
80	18	920	340	23	660
100	21	900	360	21	640
120	24	880	380	18	620
140	27	860	400	16	600
160	28	840	420	13	580
180	30	820	440	10	560
200	31	800	460	6	540
220	31	780	480	+ 3 −	520
240	31	760	500	0	500

Finally, the apparent distance of the satellites east or west of the planet's centre is found by the diagram, the unit of distance being the planet's equatorial radius. The sums I, II, III, IV, are in units of one-thousandth of the circumference. They may be reduced to degrees by multiplying by 0·36.

XUM

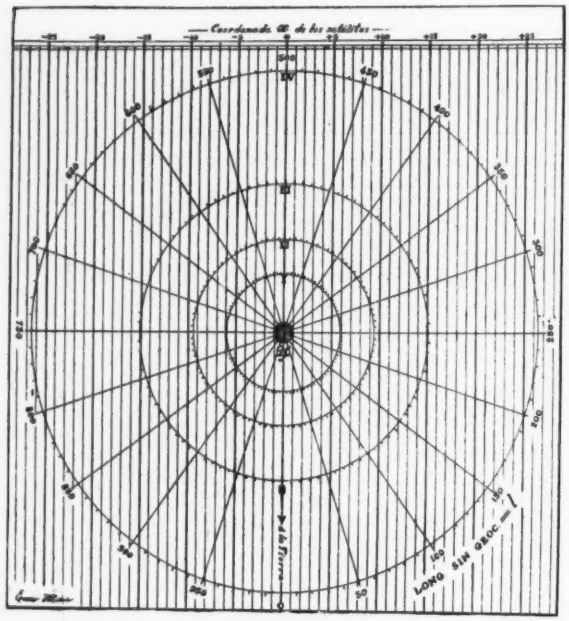


FIGURE 424.

If I°, II°, III°, IV° denote their values in degrees, we may instead of using the diagram find the distances from Jupiter by the equations.

Distance of I = 5·93 sin I°
" " II = 9·44 sin II°
" " III = 15·06 sin III°
" " IV = 26·49 sin IV°

Satellites are west of Jupiter when their angle is less than 500, or 180° after reducing to degrees. The original tables contain a correction for light-time, but I have omitted this. The quantities given here are for a mean light-time. The tables also enable us to find the times of conjunction or opposition of Jupiter. These phases occur when m is 500 and 0 respectively. For example to find when Jupiter is in opposition in 1913.

	Arg.	m.	n.
Table A_1	19	0	0
" A_2	13	659	736
" A_3	July	300	28
" A_4	17	40	4
Sum	—	999	768
Correction Table B.	768	31	—
Corrected m... ...	—	30	—

And since m increases thirty in twelve days (Table A_4), we must go twelve days earlier to obtain the date of opposition, which therefore occurs 1913, July 5th.

BOTANY.

By PROFESSOR F. CAVERS, D.Sc., F.L.S.

SHORTENING OF WINTER-REST OF TREES.—In recent years much attention has been paid to methods of forcing based upon the awakening of activity in dormant plants by means of warm water or anaesthetics.

The etherisation of bulbous and other plants for the cut-flower trade has, largely owing to the experiments of Johannsen, been successfully employed with lilac, mimosa lily of the valley, and so on, and has proved remarkably economical of time, space, and heat. The plants are exposed for a day or two in a tight box to an atmosphere of ether vapour, and after treatment are placed under conditions favourable for growth.

Gardeners have long known that placing the roots of plants in warm water tends to start into more rapid and certain growth dormant plants, especially when transplanting them. Molisch has described interesting results based upon the warm-bath method of forcing, which consists essentially in immersing the plant or branch in water at a temperature of 30° to 35° C. for from nine to twelve hours, as a rule. When potted plants are used, it is preferable to invert the pot and immerse the stem only, since the roots are usually more sensitive to injury.

Jesenko (*Ber. deutsch. bot. Ges.*, XXX, 1912) has recently described a series of experiments with various trees and shrubs, in which different substances were used in order to shorten the normal resting-period. He obtained successful results with immersion in alcohol and various acids, besides water charged with carbon dioxide and plain water. He finds that solutions of alcohol and acids gave better results in the middle of the resting-period than towards its close, and that immersion in a strong solution for a short period has the same effect as immersion in a weak solution for a long period. It is suggested that the action of the solutions used is not due merely to a stimulus in the strict sense of the word, but that the solutions employed set up chemical processes in the plant exposed to them. This view is supported by various observations; for instance, good results were obtained by injecting the solutions into the twigs instead of immersing the latter in the solutions.

Burgerstein has published (*Progressus rei botanicae*, IV) a concise but useful summary of the various new methods used in forcing. After describing the anaesthetic and warm-bath methods, in connexion with the researches of Johannsen, Molisch, and Howard, he deals briefly with the forcing effects of frost and partial desiccation. He points out the apparently paradoxical results of these modern experiments, which show that the resting-period can be curtailed by cooling or by warming; by supplying the plant with warm water or by depriving it of water by placing it in warm dry air; or by treating it with narcotic vapours like ether or chloroform. He concludes by stating that practically nothing can be said at present in answer to the questions, *how* these various methods of treatment act upon the plastic substances of the plant, why it is easier to wake the plant up at an *early* stage in its "sleep" than at a later stage, and so on.

OPHIOGLOSSALES AND MARATTIALES.—Professor D. H. Campbell, of the Leland Stanford University, California, has kindly sent the present writer a copy of his recent great work on the lower ferns ("The Eusporangiatae": Carnegie Institution, Washington, 1911) — a fine quarto volume of two hundred and twenty-four pages, with thirteen beautiful plates and nearly two hundred text-figures.

For over twenty years, Professor Campbell has studied the Pteridophyta, or fern-alliance, and has published a large number of memoirs on these plants, as well as some on the liverworts, and his "Mosses and Ferns" is a standard work for the student of the Byrophyta and Pteridophyta. In this sumptuous volume on the "Eusporangiatae," he brings together a summary of the present state of knowledge concerning the small but important families, Ophioglossaceae and Marattiaceae, which are placed at the base of the fern series in the modern scheme of classification.

Until comparatively recent times it was held that the ferns in which the sporangium is developed from a single cell ("Leptosporangiatae") gave rise to those in which the sporangium comes from a group of cells ("Eusporangiatae"), on the ground that in this character at any rate the former were simpler. It was held that the fertile spike of *Ophioglossum* arose by fusion of numerous small sporangia, and by further reduction and contraction the cone

of Lycopods was derived from the same ancestry. The revolt against this view was led by Campbell himself, and the opposite position is now accepted. In his great series of memoirs on the Pteridophyta in the *Philosophical Transactions*, Bower showed that the terms "eusporangiate" and "leptosporangiate" must be abandoned as the names of groups, though useful in a purely descriptive sense. In Bower's classification, the small family Ophioglossaceae is separated, as Ophioglossales, from the remaining ferns which are classed as Filicales. The Filicales are divided into three main groups of families according to the characters of the sporangial clusters (sori) and of the sporangia themselves, the three groups being termed "Simplices," "Gradatae," and "Mixtae." In the Simplices (including Marattiaceae, Osmundaceae, and others) the sporangia of a sorus arise simultaneously; the mechanism for dehiscence of the sporangium is slightly developed, and the spore-output per sporangium is large. In the Gradatae (Hymenophyllaceae, Cyatheaceae, and others) the sporangia are produced on a more or less elongated rod-like placenta, and are developed on this in basipetal order (the oldest above, the youngest below), while the dehiscence mechanism is well-developed and the spore-output is smaller than in the Simplices; in the Mixtae, the sporangia of a sorus arise in no definite order; hence the sorus contains sporangia of various ages mingled together, the mechanisms for dehiscence and usually also those for protection of the sporangia are more perfect, and the number of spores per sporangium is usually restricted to sixty-four or sometimes forty-eight.

Campbell, however, points out in his memoir that the isolation of the Ophioglossaceae, as Ophioglossales, on one hand, and the inclusion of Marattiaceae with the remaining ferns in the Filicales, on the other, hardly do justice to the close affinities that exist between the Ophioglossaceae and the Marattiaceae. The author's detailed and skilful presentation of the facts of structure and development in these two families make this one of the most important botanical memoirs published in recent years. In the case of each family, he describes and compares the structure and development of the several genera, dealing in turn with the germination of the spore, the prothallus and sexual organs, the embryo, and the young and adult stages of the sporophyte, thus completing the life history.

The greater part of the volume is based upon the author's own work, the material for which has chiefly been collected by himself in various parts of the Tropics. In the case of every form worked at, various gaps in former accounts have been filled, the result being that we have here perhaps a more complete picture than exists at present for any other group of the higher plants. After a survey of the morphology of the two families, the author brings out clearly the strength of the links connecting these families. On various grounds he concludes that of the three genera of Ophioglossaceae, *Ophioglossum* is decidedly the most primitive, while *Helminthostachys* on the whole comes nearest the Marattiaceae, the simplest and presumably most primitive genera of the latter family being *Kaulfussia* and *Danaea*, while *Marattia* and *Angiopteris* are the most specialised.

The author believes that from some form allied to the simpler existing species of *Ophioglossum* the whole fern series has arisen; that in this whole series the leaf is the predominant organ, the stem at first being of quite subordinate importance; that this ancestral fern was one-leaved, the leaf being at first a fertile (spore-bearing) structure, perhaps without any definite sterile segment; and that from this central type several lines diverged, of which only a few fragments persist.

SCOTTISH PEAT DEPOSITS.—During the last seven years F. J. Lewis has published the results of his elaborate investigation of the plant-remains in the peat deposits of Scotland, and in his concluding paper (*Trans. Roy. Soc. Edinburgh*, XLVII) he gives a summary of the sequence of the layers found in these deposits, as follows:—

1. *First Arctic Bed*, probably corresponding to Geikie's Fourth Glacial Stage, with ice-sheets and valley glaciers, arctic climate with snow-line from one thousand to one thousand five hundred feet; in the Hebrides and Shetlands the beds contain dwarf willows, birch, crowberry, various temperate water-plants, and so on.

2. *Lower Forestian*, Fourth Interglacial Stage, consisting of forest overlying morainic accumulations of the Fourth Glacial Stage, genial climate, land area of greater extent than now; the buried forests, seen in the southern uplands as well as in the Hebrides and Shetlands, contain birch, hazel, and alder—showing that the now treeless West Shetlands had a calm and genial climate.

3. *Lower Peat Bog*, with *Sphagnum*, cotton-sedge, *Molinia*, and so on, and in the lower layers also *Phragmites*, *Equisetum*, *Menyanthes*.

4. *Second Arctic Bed*, widely distributed, with willows, crowberry, *Loiseleuria*, *Arctostaphylos alpina*, birch, *Lychnis alpina*, and so on.

5. *Upper Peat Bog*, still more widely spread, and similar to the lower bog in general characters. The Lower and Upper bogs, with the intervening Second Arctic Bed, probably answer to Geikie's Fifth Glacial or Lower Turbarian Stage of valley-glaciers, with average snow-line at about two thousand five hundred feet, and cold, wet climate.

6. *Upper Forestian*, or Fifth Interglacial Stage; in the south this upper forest consists chiefly of Scots pine, replaced in the north and at high altitudes by birch, and extends to over one thousand feet higher than the present limit of trees; the climate was relatively dry and genial.

7. *Recent Peat*, Sixth Glacial or Upper Turbarian Stage, with high level glaciers, snow-line at three-thousand five hundred feet, climate rather cold and wet.

LIFE HISTORY OF *PYRONEMA*.—A large amount of cytological work has been done on the Ascomycetes since Harper showed, in 1895, that in the mildew *Sphaerotheca castagnei* two nuclear fusions occur in the life-cycle—the first in the female cell or oögonium and the second in the young ascus. In 1900 (*Ann. Bot.*) Harper published a long paper based largely on his work with the small ascomycetous fungus *Pyronema confluens*, in which he claimed that the nuclei of the antheridium and oögonium—the male and the female nuclei—fuse in pairs, the fusion-nuclei passing into the ascus-producing threads; in the young ascus, as in *Sphaerotheca*, a second nuclear fusion occurs, preceding the division into the eight nuclei of the developing ascospores. A voluminous and somewhat controversial literature has resulted from the extension of this line of work to other fungi.

Claussen has now (*Zeitschr. für Bot.*, 1912) published an elaborate paper, beautifully illustrated by six double plates, on the life history of *Pyronema confluens*, together with a critical commentary on the results of other workers. He finds that the male nuclei enter the oögonium and pair with the female nuclei, though no fusion occurs between the paired nuclei. These pass out into the ascogenous hyphae and there undergo division, still remaining paired but not fused. Finally, the descendants of these nuclei fuse in the young ascus. In the young ascus there is a pair of nuclei, one male and the other female, and these divide into a paired nucleus for the ascus and two reserve nuclei; the paired nuclei fuse, giving one nucleus, and this divides with reduction of chromosomes (heterotypic division). Hence, in the life cycle there is a single fusion and a single reduction division. The sporophyte generation, represented by the ascogenous hyphae, is therefore not sharply separated from the gematophyte generation; its nuclei are paired, the double number of chromosomes being present in the coupled nuclei and not in a single nucleus. The young ascus represents the spore mother-cell, its fusion-nucleus containing as many double chromosomes as the gametophyte nuclei contain single ones. A similar pairing of sexual nuclei, without fusion until a relatively late stage in the life cycle, occurs in the Uredineae, as shown by Blackman and others.

PHYSIOLOGY OF TREES.—Some interesting results have recently been obtained by Ramann and Bauer (*Jahrb. für wiss. Bot.*, 1911) from investigations on a large scale of the changes in dry-weight and ash-composition in the saplings of a number of trees. In spring, during the expansion of the

XUM

leaves, there is a loss of from twenty to forty-five per cent. of the total dry weight in the case of deciduous trees. This remarkable result emphasizes the great expenditure of energy and material during growth. In the case of coniferous trees, however, there is a much smaller loss, or even a small gain, which may be explained by the fact that these trees are able to make new food by means of their old leaves, at an early period in spring, and thus make good the loss by respiration during the opening of the buds. It is suggested that in spring the abundance of soil-water and stored food lead to a sort of temporary overfeeding in deciduous trees, and to this they consider the large-celled character of the spring-wood is related. The second outburst of growth which often occurs later in the year, is also caused frequently by abundant supply of water, and in this case also a ring of "spring" wood is produced. In pines growing on rich low-lying moors, the wood is practically all of the same character as the spring wood; in such a habitat water and food-substances are abundant throughout the growing season.

From extensive ash analyses it was found that, excepting in the conifers, practically no nitrogen was absorbed during the spring, while the leaves were expanding. The time of maximum absorption of nitrogen varies according to the species, then falls off in late summer; in the Alder, owing to the presence of the root-nodules with their nitrogen-fixing bacteria, the absorption of nitrogen continues steadily from May to November. The time of maximum absorption of each of the elements—potassium, calcium, magnesium and phosphorus—varies in different species; while in the same species the various elements are absorbed at different times and at rates which vary independently. For instance, the pine absorbs nitrogen most rapidly in June, calcium in August.

Zon and Graves (*U.S. Dept., Agric., Forest Service Bulletin No. 92*, 1911) have studied the influence of light on the growth of trees. Their admirable memoir deals with the different kinds of light—direct, diffused, overhead, lateral, reflected. Diffused light is the most important, but some trees need direct as well as diffused light, either during their whole life or at the time of leafing and of flowering. A table is given showing the decrease of direct and diffused light with increase of latitude, direct light decreasing most until at the poles it is zero, whereas diffused light is 20; at the equator direct light is 489 as against 227 for diffused light. The authors also discuss the variation of direct and diffused light quantities with altitude, and the minimum light needed for various trees, but the greater part of their paper is devoted to the question of shade-tolerance, or the ability of trees to endure shade, and the way in which this tolerance is influenced by climate, altitude, soil moisture, soil fertility, and the age and vigour of the tree. The methods of determining tolerance are discussed under three headings:—(1) empirical methods—observations on density of the crown, amount of branching, and so on; (2) anatomical and physiological methods—minute structure and assimilation capacity of the leaves; (3) physical methods —measurement of luminous and chemical light intensities.

Preston and Phillips (*Forestry Quarterly*, 1911) have enquired into the nature and variation of the reserve food materials in certain American trees, comparing their results with those obtained by European investigators. Starch seems to be the chief reserve food, and in temperate climates a great reduction in its amount occurs during the first weeks of winter, though there is no great increase in the amount of sugar except at the unfolding of the buds in Spring. The maximum for reserve starch in deciduous trees appears to be at the period of leaf-fall, while in evergreens it is at the opening of the buds in Spring.

CHEMISTRY.

By C. AINSWORTH MITCHELL, B.A. (OXON.), F.I.C.

RED PHOSPHORUS.—The *Berichte* of the German Chemical Society (1912, XLV, 1514) contains an interesting account of experiments made by Messrs. Stock, Schrader and Stamm upon the conditions for converting ordinary phosphorus into the red modification by means of radiation. The influence of red rays and ultra-violet rays was very slight, the greatest effect being produced by visible rays in the violet part of the spectrum. When exposed to radiations of a mercury-vapour lamp or an induction spark the phosphorus changed through yellow to red and subsequently became opaque, probably owing to the coagulation of the colloidal solutions first formed.

The red phosphorus formed under the influence of radiation ignited at 430°—440° C., ordinary phosphorus probably being produced before ignition. The darker the specimen the greater was its specific gravity, which ranged from 1·95 to 2·25; and considerable differences were also observed in the various preparations as regards the rate with which they would combine with oxygen. Apparently they were all amorphous in structure.

When ordinary phosphorus vapour was heated to a temperature of about 1000° C. and then suddenly cooled it yielded an amorphous red modification, whereas when the cooling was done slowly there was no such change. The explanation suggested is that the high temperature causes dissociation of the P_4 molecules, and that the sudden chilling causes the resulting smaller molecules to combine together or with undissociated P_4 molecules to produce a red modification.

This phosphorus is red and transparent in thin layers, but appears violet-black in large masses. It has a somewhat lower specific gravity than ordinary red phosphorus, and is also more permanent when exposed to the air. It also offers great resistance to the action of boiling sodium hydroxide solution.

CHEMICAL REACTIONS AT HIGH PRESSURES.—An apparatus has been devised by Dr. F. Bergius (*Zeit. angewan. Chem.*, 1912, XXV., 1171), for studying the process of chemical reactions under pressures maintained at over one hundred and fifty atmospheres for several weeks and at high temperatures (300° to 400°C.). Under these conditions carbon will react with water at 350°C., in the presence of a catalytic agent, with the formation of hydrogen and carbon dioxide. Aromatic hydrocarbon hydroxyl derivations, such as naphthol and phenol were obtained in the same way at a pressure of one hundred atmospheres by the interaction of solutions of alkalies upon the corresponding hydrocarbon chlorine derivative; while oxygen could be made to combine directly with calcium oxide to form calcium peroxide.

But perhaps the most interesting reaction was the production of an artificial coal of very similar composition to natural coal. This was obtained by heating either peat or cellulose with water to about 340°C. at a high pressure. This method also seems likely to throw light upon the mode of formation of petroleum and its derivatives.

DISTRIBUTION OF NITROGEN IN WHEAT.—A series of estimations of the distribution of nitrogen in the different parts of the wheat grain has been made by Messrs. Greaves and Stewart (*Journ. Agric. Science*, 1912, IV, 376), the wheat being ground in a small experimental mill. From the results obtained with fifty-eight different varieties the conclusion is drawn that the amount of nitrogen in the whole wheat does not afford a measure of the quantity that will be left in the flour. Thus, the proportion of proteins in the flour ranged from 56·84 to 65·56 per cent. of that originally present in the grain, while the proportion in the bran showed variations of 25 to 32·7 per cent. Wheats rich in protein yielded flours containing no more nitrogen than those that contained relatively little protein. From the average results obtained with forty-two varieties of wheat it was calculated that the proteins were distributed between the flour, bran and "shorts" in the respective proportions of 61·87, 27·98 and 9·92 per cent.

DRYING OF YEAST.—Hitherto processes used for drying yeast have inevitably caused the destruction of a large proportion of the living cells and the dried product has had much lower enzymic activity than the fresh yeast. In a process recently described by Herr Hayduck (*Zeit. angewan. Chem.*, 1912, XXV, 1179) this drawback is overcome and dried yeast with ninety per cent. of its cells alive is obtained.

The yeast is first suspended, for two or three days, in water through which a current of air is passed, the effect of this being to alter the proteins of the cells in such a way that they may be subsequently dried without injury. Another process is also used, in which the yeast is pressed, mixed with sugar and dried at a temperature of 59°C.

GEOLOGY.

By G. W. TYRRELL, A.R.C.Sc., F.G.S.

THE NEW MADRID EARTHQUAKE.—The succession of shocks designated collectively the New Madrid earthquake began on December 16th, 1811, in an area of the Central Mississippi valley, and lasted for more than a year. For continuity of disturbance, area affected, and severity, this series of shocks deserves a place among the great earthquakes of the world. Scientifically, this earthquake, occurring in a low-lying alluvial region, may be regarded as a type exhibiting in unusual detail the geological effects of great disturbances upon unconsolidated deposits. Even after the lapse of a century the effects of the earthquake may still be studied. A systematic record of the phenomena is made, for the first time, by M. L. Fuller, in Bulletin 494 of the United States Geological Survey. The effects of the initial earthquake of the series are thus graphically described:—"The ground rose and fell as earth waves, like the long, low swell of the sea, passed across its surface, tilting the trees until their branches interlocked, and opening the soil in deep cracks as the surface was bent. Landslides swept down the steeper bluffs and hillsides; considerable areas were uplifted, and still larger areas sunk and became covered with water emerging from below through fissures or little 'craterlets,' or accumulating from the obstruction of the surface drainage. On the Mississippi great waves were created, which overwhelmed many boats and washed others high upon the shore, the return current breaking off thousands of trees and carrying them out into the river. High banks caved and were precipitated into the river, sand bars and points of islands gave way, and whole islands disappeared." Fortunately the area was very thinly settled; and the houses, for the most part built of logs, did not collapse suddenly, but gave the inhabitants sufficient warning for them to escape. Hence there was only a slight loss of life.

A remarkable feature of the earthquake was the formation of long canal-like depressions, which appear in reality to be narrow, down-faulted blocks between two parallel cracks. Similar fissuring in the river banks and in the higher bluffs bordering the Mississippi lowlands resulted in great landslides. A regional warping of the surface also occurred, giving rise in some places to broad dome-like lifts; in others to depressions, now mostly occupied by peculiarly-shaped lakes, such as Reelfoot Lake, Tennessee. Many of these lakes contain trees still standing, but killed by the century-old submergence. Some striking photographs help the reader to realise the clearness with which the effects of this earthquake may still be traced.

METEOROLOGY.

By JOHN A. CURTIS, F.R.MET.SOC.

THE weather of the week ended July 20th, as set out in the Weekly Weather Report issued by the Meteorological Office, was fair and dry at first, but became unsettled over England generally, and rain was experienced in most localities. Thunder was heard on the 14th, and there was a thunderstorm at Alnwick Castle on the 20th. Temperature was above the average in all districts except England, N.E., and Ireland, N. The greatest excess was 4°·3 in England, S.W. Maxima exceeding 80° were observed in all districts except Scotland, N., and in Ireland. The highest reading was 90° at Camden Square on the 15th, with 88° at Greenwich, Tottenham, Bath and Clifton. The lowest of the minima were 37° at Kilmarnock, and 38° at Balmoral, West Linton and Marlborough. On the grass the temperature fell to 33° at Marlborough and at Wisley. The temperature of the soil, both at one foot and at four feet was above the average.

Rainfall was greatly in defect very generally. The only district where the amount collected was in excess was England, N.E., where it was 0·19 inch above normal. In Scotland, E., and England, N.W., the week was almost rainless. At only one station in the United Kingdom did the total rainfall for the week amount to one inch or upwards, namely, at Alnwick Castle, 1·63 inches. Sunshine was in excess except in Scotland, E., and England,N.E. The defect of 0·3 hours daily (2%) in Scotland, E. is noteworthy in face of the fact that the rainfall in that district was only 0·01 inch or 0·72 inch below the average.

The mean temperature of the sea water round the coast varied from 52°·2 at Lerwick to 67°·4 at Margate.

The weather of the week ended July 27th was mostly unsettled and rainy, with frequent thunderstorms. Temperature was below the average in Scotland, E., England, N.E., Ireland and the English Channel, but above it elsewhere. The greatest variation was in England, E., where the mean, 63·1, was 2°·4 above the normal. The maxima were much lower than in the previous week, and exceeded 80° only at Greenwich (82°), and at Camden Square (81°) on the 24th, the next highest reading being 79° at Margate on the same day.

In Ireland, N., the highest reading was only 68°. The lowest readings of the week were 31° at Balmoral on the 23rd, and 35° at Glencarron on the 25th. The lowest readings on the grass were 29° at Balmoral and 34° at West Linton.

The temperature of the soil did not differ much from the average.

Rainfall was in excess except in Scotland, N. and W., and in England, S.E. In many cases the excesses were large, and in Ireland, S., the total was three and a half times the usual amount. At each station in this district rain fell on each day, and the total collected at Cahir was 3·94 inches.

Sunshine was in defect in all districts, by as much as 3·7 hours per day (23%) in England, N.E., and 4·0 hours per day (27%) in the English Channel. In seven districts, namely, Scotland, E., England, N.E. and S.W., Midland Counties, the English Channel and Ireland, N. and S., the amount of sunshine was less than half the normal.

The mean temperature of the sea water was, as a rule, above the average, and varied from 52°·4 at Lerwick to 65°·6 at Eastbourne.

The weather of the week ended August 3rd was very dull and wet, with many thunderstorms. At Alnwick Castle very large hail fell on the 29th of July.

Temperature was below the average at every station. The district values were in defect by amounts varying from 3°·1 in England, E., to 5°·8 in Ireland, S. The highest readings reported were 72° at Greenwich, 71° at Camden Square, and 70° at Scarborough, Yarmouth and other stations. The lowest readings were 30° at Llangammarch Wells, 31° at West Linton and 33° at Markree Castle. On the grass the temperature fell below the freezing point at many stations, the lowest reading reported being 25° at Birmingham. The temperature of the soil at one foot depth was below the normal; but at a depth of four feet the readings were mostly above the average. Rainfall was in excess in all districts; very greatly so in some. In England, N.W., and S.W., and in the English Channel, the total collected was more than three times as much as usual. Sunshine was in defect in all districts and the average daily duration varied from 2·6 hours in Scotland, E., and England, N.E., to 5·8 hours (39 %) in the English Channel.

The temperature of the sea water varied from 49° at Lamlash to 66° at Eastbourne.

The weather for the week ended August 10th, was again very inclement, being dull, cold and wet, with many thunderstorms.

Temperature was in defect in all districts, by as much as 5°·9 in Ireland, S. In this district the mean temperature, and also the maximum and minimum, were lower than the corresponding values in Scotland, N.

The highest readings reported were 73° at Greenwich and Camden Square, 72° at Margate, and 71° at Norwich and

XUM

Cambridge. None of the minima were, however, below the freezing point, the lowest being 36° at Markree Castle and 37° at Kilkenny and Balmoral. On the grass also, no readings below freezing point were reported, the lowest of the radiation temperatures being 32° at Markree Castle and 33° at Dublin and Crathes.

The temperature of the soil, both at one foot and four feet depths, was below the average.

Rainfall was in excess in all districts, and in some parts by considerable amounts. In Scotland, E., England, S.W., and the English Channel, the total was about three times as much as usual. Sunshine was in defect at every station, and in Scotland, E., the mean daily amount was only 0·8 hours, as compared with an average of 4·9 hours, or one-sixth of the usual amount.

The mean temperature of the sea water round our coasts ranged from 52°·7 at Berwick to 63°·2 at the Shipwash light vessel.

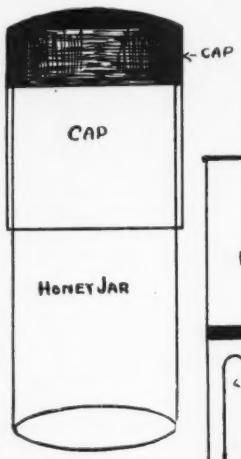


FIGURE 425.

OCEAN WAVES.—In the monthly meteorological chart of the Indian Ocean and Red Sea for August, some very interesting particulars are given of high ocean waves. It appears that the abnormally high solitary sea is the most dangerous, and that these are not confined to tropical seas, but are occasionally met with near our own coasts. Thus, in 1897, at the entrance to the English Channel, the steamship "Millfield" encountered a sea which washed overboard the upper bridge, the funnel and the boats, and had her fires extinguished by eight feet of water, which entered by the openings on deck.

Another case mentioned is that of the "Brandenburg," which shipped a tremendously high wave, estimated at sixty-five feet in height, which stove in the crow's nest constructed of quarter inch steel plating at fifty feet above sea level!

MICROSCOPY.

By F.R.M.S.

A NEW POND COLLECTING APPARATUS.—For collecting pond and marine life it is necessary to have some appliance which shall be simple, efficient, portable, and if possible, cheap and easy to clean. The first three or four of these requirements are obviously essential and desirable, but the last also is an essential as we may on one occasion be studying ordinary pond life, on another it may be marine life, whilst on yet another we may be engaged in examining the vegetable or bacterial organisms existing in, say, a filter bed or its effluent. This last is a most important department of research, and requires considerable care if reliable results are to be obtained. Many, indeed, use the ordinary Wright's bottle, which, however effective in most cases, is of but little use when bacterial life is abundant. Others use a funnel plugged with cotton wool, but this soon gets clogged with the fine ooze which predominates in a filter bed.

A very simple and inexpensive arrangement may, however, be contrived which answers well not only in all ordinary cases, but also for collecting even such minute organisms as diatoms and bacteria. The use of it for some time compels the author to give the whole his warm recommendation as fulfilling every requirement (see Figure 425).

It consists of three parts, the first of these being a common honey-bottle six inches long by two in internal diameter. The kind I use is called Gray's Honey jar which is of the same internal diameter throughout and can be purchased at Mr. George Rose's, 30, Bolton Street, Liverpool, at the cost of twopence each. Half a dozen of these should be obtained, costing with postage about eighteenpence. They are portable, which cannot be said of the ordinary collecting bottle, and three or four can easily be carried in the coat pockets. They are fitted with a good cork wad and a screw cap which secures the contents perfectly without any leakage—an advantage of some value when investigating sewage life.

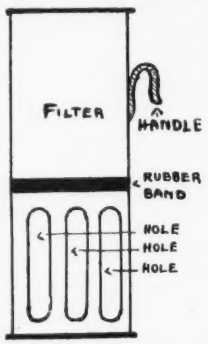


FIGURE 426.

Over the cap fits loosely a tin cup about two or three inches deep, and just wide enough to go freely over the cap or bottom of the jar. It has no handle, and only one is needed even if we carry more than one honey jar on our expeditions. It is obviously easy to clean. It may, however, be dispensed with altogether if required. Mine cost sixpence at the tinman's. The top is "wired," not left sharp (see Figure 426).

The filtering apparatus itself, upon which the efficiency of the whole depends, consists of a simple tin tube, five inches long by one and three-quarter inches external diameter. It is open at both ends, which are both wired. On the lower half numerous slots or circles are cut about half an inch in diameter, as shown in the diagram. A piece of curved spring is soldered to the side, and the whole should be of such a diameter as to fit without stiffness into the honey jar. Over the bottom, about three inches up the sides, is stretched a piece of the finest silk gauze, or better still, a piece of what is known as 160 bolting silk, which can be obtained at Messrs. C. Baker's in Holborn, and doubtless of other opticians. For bacteria and diatoms this may be replaced by a bit of fine Japanese undyed silk. An indiarubber band (mine is made from an old bicycle inner tube) securely fastens everything in place.

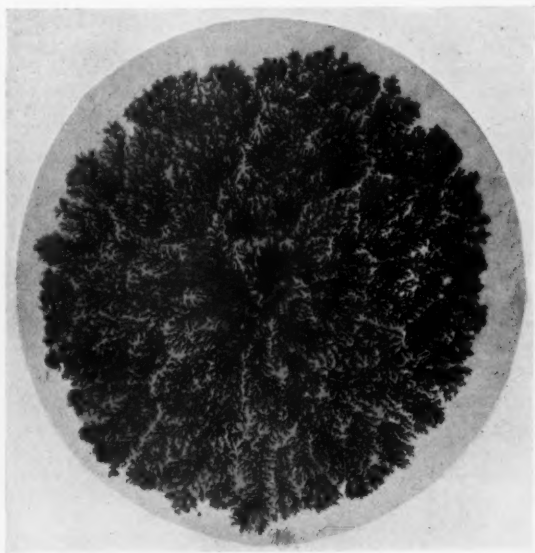


FIGURE 427.

Thus equipped we go forth, and on arriving at the scene of operations, remove the cup and cap and draw out the filter, which we suspend by its hook so as to hang *outside* the jar. We then proceed to work, and by means of the cup fill the filter time after time, until we judge that enough has been gathered. We then empty the filter into the jar and recommence. It is, however, a great mistake to overcrowd the jar, and very cruel, too. A large amount of life is lost, and many rotifers will not work in such unnatural surroundings. When finished, unhook the filter, replace it in the jar, and put on the cap and cup after wiping the latter. As we have already said, the cup may be dispensed with, the filter being held in the one hand and the jar used as a ladle.

With care in the selection of the straining fabric, comparatively few living things will be lost, as they are not so crowded together as when passing down a narrow tube. The fabric

may be washed and used several times, but it ought to be boiled before its use a second time. The whole answers almost every requirement of even the most exacting, and its cheapness is its smallest recommendation. The ordinary wire ring and gauze funnel with a small tube at the bottom is, as everyone knows who has used it, a mere inconvenient plaything, and for sewage effluents all but useless. This apparatus, simple as it is, answers all but every scientific requirement. It catches an exceedingly large proportion of bacterial organisms, and for ordinary pond life it is an almost perfect filter for very few creatures get through.

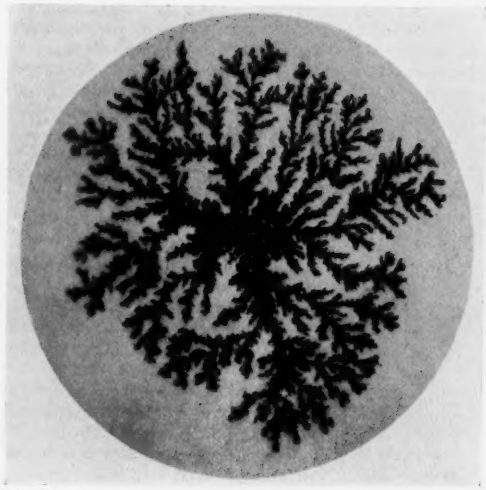


FIGURE 428.

For marine life, one of a much larger size may be used and the dredge emptied into it, but in this case it is best made without openings in its side, and a wet string must go tightly round the bottom not less than three or four times and be securely fastened. One a foot wide and high is a good size. It need not have a hook on the side, as a bucket will, of course, be used into which to empty the filtrate. A quart enamelled jug makes a convenient ladle, and it must be remembered that marine life above all requires plenty of room, and that large glass *cloches* must be employed if an aquarium is not used. Finger glasses, which do well temporarily for pond life, are utterly inefficient for any marine gatherings except those derived from the small rock pools.

My own apparatus cost me thirteenpence. My old apparatus, which cost about five shillings, is now used as a pickling jar, a purpose for which it is seemingly far better fitted than that for which it was primarily designed. People no longer ask me whether I have been catching "minners," in fact they see nothing, not even a collecting stick, and hence one is looked upon as slightly more sane than heretofore—so easily are reputations for wisdom acquired. I am quite certain that once used the apparatus will never be replaced, at any rate not by any other at present in the market.

E. ARDRON HUTTON.

SPOTS IN PHOTOGRAPHIC PRINTING PAPERS.—Objects of much interest to the microscopist are the minute metal spots which occur in various kinds of photographic papers, chiefly the printing-out varieties, coated with gelatino-chloride and collodio-chloride emulsions.

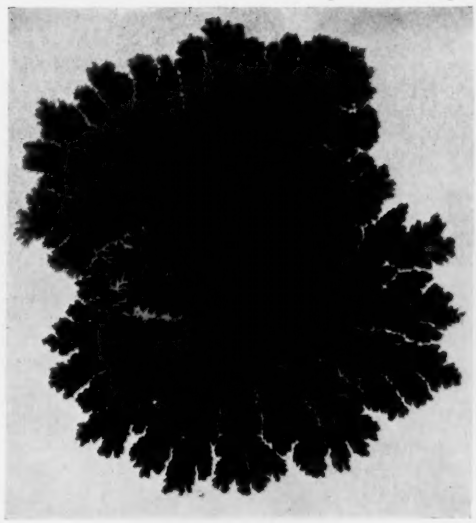


FIGURE 429.

If the sensitiveness of a sheet of paper be removed by the usual fixing process and the surface examined under a low power—a one-inch objective is sufficient—one will frequently find a few spots embedded in or resting upon the film. The larger ones can be detected by unaided sight, but the more minute forms require optical assistance to locate them.

Many of the spots have no definite shape, but occasionally most beautiful designs occur consisting of a net-work of filaments, radiating from a dense nucleus. They are doubtless the result of crystallisation of one of the various metals entering into the composition of the emulsion—probably the silver—as the formation of the filaments bears some resemblance to the well-known shape of silver crystals.

These "silver trees" are generally caused by metallic impurities in the paper itself as, in several cases, the nucleus was found embedded in the fibres on the surface; small particles of metal or other reducing agents might also occur in the baryta substratum upon which the emulsion is usually coated and, in any case, an action reducing the chloride to the metallic state would result.

The sizes of the spots are sometimes as large as two millimetres in diameter and the forms range from a few straggling branches to a vast network of beautiful design. Those produced upon the collodion film are characterized by the delicacy of the filaments which can be traced to the exact point in the nucleus where the action commenced; in the case of the gelatine film, the filaments are much thicker and the nucleus is generally a larger mass than in the collodion variety. The difference may be due to the dissimilarity of the gelatine and collodion or to a combination of metals forming the crystals.

FIGURE 430.

Figures 427 and 428 show types found in the collodion film, and are magnified eighty and fifty times respectively, the photographs being taken by reflected light, which is the best method of illumination for examining this variety.

Figures 429 and 430 are typical of those which occur in gelatine film magnified ninety and ninety-five times respectively; in these instances transmitted light was used, as reflected light fails to reveal the narrow intersections between the filaments.

The colour of the filaments is always light brown when they are embedded in the film, and most of the collodion variety occur in this position: in the case of the gelatine papers, most of the crystallisation takes place upon the surface. With both

types, the crystals assume a dead black color when they are upon the top of the film.

In Figures 249 and 430, nearly the whole of the action took place on the surface, as the images seen through the microscope were black excepting some portions at the edges which were within the film and consequently of a brown color; this difference will be noticed in the illustrations.

The chemical reduction probably happens when the emulsion dries, or soon afterwards, as the spots have been found in paper of comparatively fresh make; some paper has been kept for a long period to ascertain if the action occurs or increases with age, but neither feature was noticed.

The unending variety of the designs and their minute structure make them worthy of attention from all users of the microscope.

C. A. B.

ORNITHOLOGY.

By WILFRED MARK WEBB, F.L.S.

ORNITHOLOGY AT THE BRITISH ASSOCIATION.—At the British Association meeting, held at Dundee, a considerable amount of interesting work on birds was recorded. In the final report of the Committee dealing with secondary sexual characters in birds [presented to Section D (Zoölogy)] among other matters the cause of sterility in hybrids was considered, and the following account was given:—An attempt was made to rear hybrids between the common pheasant and the jungle fowl, but the incubation of about sixty eggs resulted in the hatching of a single chick, which died owing to a cerebral hernia two days after hatching. This chick on dissection proved to be a male, and the reproductive organs were in a perfectly normal condition for a chick of that age, showing no degenerative or retarded development. Three hybrid male pigeons (hybrid between domestic dove and pigeon) were obtained from a pigeon-fancier. These birds were kept for about a year and were paired with female pigeons successfully, and the eggs were incubated in the normal manner by both parents. In all cases the eggs were sterile. The three hybrids were killed and dissected, and their spermatozoa and testes, which on inspection appeared quite normal, were examined histologically. On comparison with normal doves and pigeons, it was found that the great majority of the spermatozoa of the hybrids were twice the normal size, and this abnormality of size was traced to the fact that the second maturation division was entirely suppressed. The abnormality was traced further back to the first maturation division, where it was found that the chromosomes, instead of forming the ordinary eight synaptic groups, were irregularly fragmented and scattered on the mitotic spindle, some of the chromative masses being much smaller, others much larger, than the normal synaptic chromosomes. Previous to this division it appeared that the spermatogonia in the testes were perfectly normal, so that we must ascribe the abnormality of the spermatozoa, and the consequent sterility of the hybrids, to the incapacity of the chromosomes derived from the two parents to form synaptic pairs. These results will shortly be published in detail in *The Quarterly Journal of Microscopical Science*.

Further observations on sterile hybrids are being made in the case of some birds presented by Mrs. Haig Thomas, which have been kept for varying times in aviaries and some of which are still alive. Investigation of the sterile male shows similar features to those observed in the case of the pigeon-dove hybrids, but other observations on sterile females and another male hybrid are not complete. The sterile female hybrids show a partial assumption of cock's plumage, and this is probably correlated with the atrophy of the ovary.

Another investigation was intended to discover if the inheritance of spurs in the domestic hen could be explained on the same lines as the inheritance of horns in horn-breeds of ewe; but the experiments have not been going on long enough to speak with any certainty.

Another experiment still in progress deals with the inheritance of an extra toe in the fowl.

Dr. C. J. Patton, who spent eight weeks at the Tuskar Light Station, County Wexford, summarised his remarks on bird migration as follows:—In such a comprehensive study as the migration of birds as carried out personally, over a considerable period of time, at an isolated rock on which a lighthouse has been built, with a lantern of powerful illumination, so many problems present themselves for investigation, and so extensive and intricate are the statistics, that in order to cover the ground at my disposal I must here confine my remarks to some special features in connection with my subject which I hope will be of general interest. To the ornithologist of a country, and especially in the case of a small one like Ireland, where a complete knowledge of the avi-fauna is not so difficult to acquire, one of the most fascinating objects is to wait and watch for species which, either unknown or of very rare occurrence in their natural habitat, are attracted and decoyed by the luminous beams of the lantern under certain meteorological phases, when on passage. It is chiefly from the lighthouse that so many birds new to Ireland have been recorded, and during my visit I was enabled to add a few more to the list. Another feature of considerable interest in dealing with bird-migration presents itself at the Tuskar light-station, namely: to what extent certain supposed desultory or mere local migrants journey? Several knotty points can, I am of opinion, be disentangled as we study this subject at such an excellent observatory as the Tuskar Rock. For any land birds which appear, even when they alight and rest a few hours, are bent on making a passage. No land birds could reside here, where fresh water to drink is unavailable, food is very scarce (for some species absent altogether), and the rock is frequently wave-swept. The third and last feature I wish to refer to is in connection with the study of variation. Splendid opportunities present themselves, because such large numbers of certain species fall victims by striking the lantern that these can be collected and preserved with facility.

A very useful discussion arose as the result of a paper dealing with the food of birds, which was communicated by Professor J. Arthur Thomson. The details of the work are as follows:—The inquiry was begun in October, 1909, under the supervision of Professor Thomson, and with the valuable co-operation of Professor Trail. The investigator, Miss Laura Florence, M.A., B.Sc., has examined about one thousand eight hundred birds, chiefly from agricultural land in the N.E. of Scotland. It is too early to draw many definite conclusions, but the inquiry shows the need for examining large numbers from different areas, and throughout the year, if trustworthy information is to be forthcoming as to the injurious or beneficial activities of common birds. Many current opinions on this subject rest on far too narrow a basis.

Birds of ninety-five species have been examined, but large numbers of any one species have not been procurable except in a few cases, such as rooks and gulls. In some cases the verdict given by previous investigators, such as Professor Newstead, has been confirmed, *e.g.*, as to the injuriousness of house-sparrow, wood-pigeon, and carrion-crow, and as to the beneficial activity of hedge-sparrow, field fare, lapwing, and plovers. On the other hand, there are several cases in which the results up to the present do not altogether confirm previous opinions; thus the diet of the black-headed gull and the common gull shows a striking resemblance to that of the useful lapwing. It is much to be desired that this inquiry, and others like it elsewhere, should be continued for a term of years; and the coöperation of farmers and others interested is solicited.

The usefulness of the investigations was emphasised by many speakers and an important point brought out was that if the waste grain eaten by birds after the harvest had been gathered in was not taken into consideration the balance might in many cases be in their favour. The need also for the making of investigations in many localities was brought forward; for a bird which is injurious in one locality may not be so in another.

Mr. Lansborough Thomson described his method of bird marking and gave some of the results, to which attention has already been called in "KNOWLEDGE"; his paper also was received with great cordiality and much commendation.

At the Conference of Delegates a short account illustrated

by lantern slides was given by the Secretary of the Selborne Society on the successful experiment in Bird Protection made during the last eight or nine years in the Brent Valley Bird Sanctuary and the nesting boxes which have been designed in connection with it.

PHOTOGRAPHY.

By EDGAR SENIOR.

PHOTO-MICROGRAPHY WITH HIGH POWERS.—In the last issue of "KNOWLEDGE," when dealing with medium power photo-micrography, we endeavoured to show that magnification beyond the degree necessary to observe distinctly all that it was possible for the objective to define, was practically useless, and that it was the aperture that determined the essential qualities required in the lens. We have an example before us which illustrates this admirably. Two objectives, one of which is a quarter of an inch having an aperture of ·95 N.A., the other a fiftieth of an inch, of ·98 N.A. Now, the former, according to theory, would be able to resolve lines or dots when so close together that ninety thousand two hundred and sixty were contained in the linear inch, and the magnifying power at ten inches would be forty diameters. With the latter the resolving power would be ninety-three thousand one hundred, and its magnifying power five hundred diameters at the same distance. The magnification is, therefore, out of all proportion to the resolving power, as without any eyepiece at all, the size of the image would be equal to, "and theoretically greater than," that obtained by means of the quarter-inch when used with a ten ocular. It, therefore, becomes difficult to understand what was gained by the use of an objective of this focus, leaving out of consideration the inconvenience

FIGURE 431. Bacilli, photographed with a Zeiss 2 mm. homogeneous immersion apochromatic objective N.A. 1·40 and a four projection ocular × 1900 diameters.

arising from its close working distance. However, as this latter objective has a larger aperture, there should be a gain in that respect. Realizing, then, the great value of aperture in microscopy, we see how essential it is that an objective should possess a wide one, when intended for use in the observation of minute objects such as bacilli, or the study of the

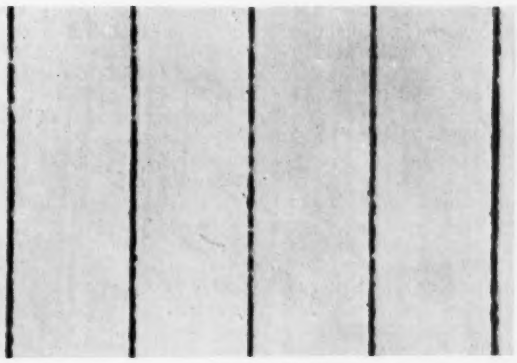


FIGURE 432.

The $\frac{1}{100}$ of a millimetre divisions of a stage micrometer × 1900 diameters.

structure in diatoms. Owing to the researches of Professor Abbe, in connection with the defining (resolving) power of the microscope, we have the general formulae $2\ \lambda$ N.A. as expressing it. That is, twice the wave length of light employed multiplied by the numerical aperture of objective. It should also be evident that anything which diminishes λ increases the resolving power of the lens, and as the wave length of light is less in a highly refracting medium than it is in air, we can understand the advantage gained by the use of homogeneous immersion objectives. The immersion fluid usually employed is cedar-wood oil, having a density of 1·515, and in order to thoroughly understand its use, we must consider the equation given by Professor Abbe, which determines the value of the numerical aperture; thus N.A. = n sin u, in which n stands for the density of the medium in which the front lens of the objective is immersed, and sin μ represents the sine of half the angular aperture. Suppose a dry objective having an angular aperture of 134° 9′ be the one considered, air being the medium in front of the lens, the index of refraction is unity, therefore n=1. Half the angle of aperture is $\frac{134° 9'}{2} = 67° 45'$. Referring to a table of sines we find that the sine of 67° 45′ is ·92554, therefore the N.A. = 1 ×0·92554. Supposing that it were possible to use this same objective with cedar-wood oil between its front lens and the cover glass, then n would equal 1·515 and the N.A. = 1·515 ×0·92554 = 1·40. We thus see the great advantage of oil immersion, and how the N.A. determines the essential qualities of the objective with regard to its ability to resolve fine structure. Photography also places a still further power in our hands, as by its means and the employment of apochromatic objectives, we are able to make use of blue light, and so obtain greater resolution still, owing to their shorter wave length. That the use of high powers requires special care to be taken in the adjustment of the apparatus is generally well known; but more particularly is this the case in photo-micrography. In order

to obtain successful results, the condenser and source of light must be carefully centred, so as to properly illuminate the object, and the auxiliary condenser employed in front of the lamp must be of such a focus that the image which it projects upon the sub-stage condenser fills the entire aperture of the latter, when the principal condition for critical illumination is effected. It then only remains to focus the image of the lamp condenser into the object field by means of the rack and pinion with which the sub-stage is provided. The adjustments of the light having been made so as to suit the requirements of wide aperture oil immersion objectives, attention has to be directed to the objects themselves. In photographing diatoms the use of an apochromatic objective with a blue light filter is to be recommended, as the shorter wave lengths will act like an increased numerical aperture, and so augment the resolving power. In the case of bacteria, however, the conditions are quite different, as we are usually dealing with stained specimens, and the filter employed to obtain the necessary contrast must be in keeping with the absorption bands of the dye used in staining. If the attempt be made to increase contrast by reducing the aperture, there is great risk of destroying fine details, as well as the formation of very marked diffraction effects round the images themselves. It is therefore evident that a deal of care is required to obtain the best results. In taking the photograph of bacteria shown in Figure 431 a Zeiss two millimetre homogeneous immersion objective of N.A. 1·40 was employed, together with a four projection ocular. The source of light was a paraffin oil lamp, and an exposure of three minutes was given, using an Imperial orthochrome plate of 200 H. and D. The specimen being stained blue, a yellow screen was used to gain contrast. It may be further mentioned that as the three millimetre apochromat of Zeiss has the same N.A. (1·40) as the two millimetre, the former would, in many cases, be the best to use owing to its greater depth of focus.

DETERMINATION OF MAGNIFICATION.—Having taken the photograph it is often necessary to know the magnification of the image. This is easily found by removing the object and replacing it with a stage micrometer, "another name for a three by one slip upon which is cemented a cover glass ruled with lines separated by intervals of known value." If the distance between the images of two of these lines seen on the focusing screen be measured, the magnification is obtained by dividing the size of the image by that of the object. This is shown in Figure 432, which is a photograph of the lines on a stage micrometer one hundredth of a millimetre apart. The distance between the lines is practically nineteen millimetres, therefore $\frac{19}{1} \div \frac{1}{100} = 1{,}900$ diameters.

EXPOSURE TABLE FOR OCTOBER.—The calculations are made with the actinograph for plates of speed 200 H. and D., the subject a near one, and the lens aperture F.16.

Day of the Month.	Condition of the Light.	Time of Day.		
		10 a.m. and 2 p.m.	9 a.m. and 3 p.m.	8 a.m. & 4 p.m.
Oct. 1st	Bright	·15 sec.	·2 sec.	·27 sec.
,, ,,	Dull	·3 ,,	·4 ,,	·54 ,,
Oct. 15th	Bright	·18 sec.	·25 sec.	·4 sec.
,, ,,	Dull	·36 ,,	·5 ,,	·81 ,,
Oct. 30th	Bright	·23 sec.	·36 sec.	·67 sec.
,, ,,	Dull	·46 ,,	·70 ,,	1·30 ,,

Remarks.—If the subject be a general open landscape, take half the exposures given here.

ZOÖLOGY.

By PROFESSOR J. ARTHUR THOMSON, M.A.

TWO-TOED HORSES.—Professor K. Skoda, of the Veterinary College in Vienna, gives a precise account of two horses which had two toes well-developed on the fore-leg. Taking the first case, each fore-leg bore beside the ordinary single digit (No. III.) a second. This showed three joints, but did not reach the ground. There was a metacarpal (or palm-bone) for this second digit, largely fused with the ordinary third metacarpal. Besides this there was the usual free splint or fourth metacarpal, and there seemed to be actually a rudimentary first metacarpal! Especially when one looks into the details of the case does it look like a reversion to a remote ancestral type with polydactylous feet.

EDIBLE SEA SQUIRTS.—It cannot be said that the natives of these islands compare well with others in the experimental study of the edible. We shrink from much that is wholesome and refuse some of our best fishes until they are filleted and re-named. We lag far behind the Japanese, for instance, in gustatory inquisitiveness, for they have learned to make an entrée of jellyfish and to make a toothsome delicacy of two species of sea squirt (*Tethyum*). Mr. A. G. Huntsman points out that very similar forms are readily available on the coasts of Canada, and he encourages experiment by precedent. "The inhabitants of Peru and Chili use as food two species of *Pyura* that occur on their coasts, and species of the genus *Microcosmus* are exposed for sale in the markets of Southern Europe." There is nothing in a name, of course, but a dish of *Microcosmus* should meet the wants of a diversity of palates. Most sea-squirts have but little muscle, and that unstriped; but in the Styelids and Tethyids the musculature is often quite thick.

NATURAL HISTORY OF SLIPPER-LIMPET.—The American limpet, or slipper-limpet, known technically as *Crepidula fornicata*, was introduced into England about 1880, and has spread rapidly on oyster grounds. Mr. Orton, who has recently studied its natural history, says that there is no doubt that it has been introduced along with American oysters, on which it fixes itself, and that the introduction is probably going on. There are many interesting features in this addition to the British fauna. It takes special care of its spawn. "It constructs about fifty to sixty membranous bags, into each of which it passes about two hundred and fifty eggs, and as the bags are made and filled with eggs they are closed and fastened together by short cords. These cords are finally all stuck on to the surface on which the slipper-limpet happens to be sitting, so that when by taking away the spawning individual the spawn is uncovered, it looks like a bundle of balloons, each containing a number of eggs." Another feature is their curious habit of sticking together in long chains, by one individual sitting on the back of another. There may be thirteen in a chain, and the bottom individuals are females, the end individuals males, and those between these are intermediate sex forms between male and female. The fact is, as Orton has shown, that the slipper-limpet is a "protandrous hermaphrodite," *i.e.*, it is first a male and then a female. The eggs develop into free-swimming larvae, which may be scattered far and wide. As the slipper-limpets feed on the same food (diatoms and the like) they compete with oysters; but it remains to be seen whether there is not plenty of food for both parties.

THE MERMIS FAMILY.—The Mermithidae form an interesting family of threadworms, not unfamiliar to those who are fond of gardening. The mature adults are found in the earth or in fresh water, and so are the first larval stages. The second larval forms are parasitic in Tracheata and Gastropods. In the earth the adults are sometimes found rolled up in a spiral or coil, sometimes male and female together. The males are smaller, thinner, more mobile, and with better developed sense-organs at the head-end. Sometimes the eggs are laid just where the female was living; in other cases, when the ground is very damp, an interesting migration occurs which many observers have noticed. The females creep up plants, *e.g.*, cabbages, probably seeking out a drier place for egg-laying. The young larvae leave the ground and bore actively into beetles, caterpillars, millipedes, slugs, and so on. It is noteworthy that no food is taken either by the adults or by the young larvae. All the feeding is done by the second larval forms during the parasitic period. Another interesting feature is that the Mermithidae differ from most other Nematodes in being able to survive mutilation and in

showing some measure of regenerative capacity. There have been two recent revisions of the family, one by E. von Daday (*Revue Suisse Zool.* XIX), and another by A. Hagmeier (*Zool. Jahrbücher* XXXII).

TOOTH-WORMS.—In some parts of Hungary there is a vulgar belief that bad toothache is due to minute worms. By their writhing and gnawing they produce abscesses in the gum, and one of the popular cures is to fumigate the mouth very thoroughly so that the worms are driven out from their hiding-places. The thorough fumigation of the mouth is a wide-spread habit all over the world, but the tooth-worm myth is rare. It may have had an origin, one might suppose, in the very rare occurrence of fly-larvae (*Sarcophaga wohlfahrti*) in bad buccal abscesses, but Professor Karol Sajo's examination of the alleged "tooth-worms," which the Hungarian sufferers are willing to demonstrate on the spot, has shown that they are the funicles of the seeds of *Hyoscyamus*, which are used, we suppose, as an anodyne.

FRESHWATER STING-RAYS OF THE GANGES.—It has been known since the beginning of the nineteenth century that rays occur high up the Ganges, but considerable doubt has existed as to the species. This has been cleared up by B. L. Chaudhuri, who shows that there are at least two freshwater Batoids in the Ganges, viz., *Trygon fluviatilis* and *Hypolophus sephen*. These species are not only found one thousand miles above tidal influence, but also breed freely in fresh water.

FISHES AND MALARIA.—In 1905, it was pointed out that the Barbadoes were remarkably free from malaria, and the reason suggested for this was that the mosquito larvae (which elsewhere grow up into distributors of the malaria organism) were devoured by a small fish, popularly known as "millions," very abundant in all the streams and pools. Captain R. B. Seymour Sewell and B. L. Chaudhuri have recently shown that there are numerous (they describe eleven species) Indian fishes which are of proved utility as mosquito-destroyers. Thus we have another thread in the web of life—"fishes may be a very important agent in regulating and diminishing the degree of malarial infection in any given district."

ERI SILK.—Messrs. H. Maxwell-Lefroy and C. C. Ghosh give an account of Eri silk, which is grown in Assam for local use, and may perhaps have a big future before it. Eri silk cannot be reeled, for the cocoon is in layers, not in a continuous thread. The moth *(Attacus ricini)* can push its way out without softening or cutting the fibres, and may be allowed to mature within the cocoon and emerge. The fibre can be spun as cotton is, and the yarn can be woven quite readily. The resulting silk cloth ("Endi") is the most durable cloth known in India, and takes dye well. The silkworms live on Castor leaves

PEARLS AND PARASITES.—After a searching inquiry, H. Lyster Jameson comes to the conclusion that there is insufficient evidence for the theory that the pearls of the Ceylon Pearl Oyster are due to the larvae of the tapeworm *Tetrarhynchus unionifactor*. It appears probable that the simultaneous presence of pearls and tapeworms in the Ceylon Pearl Oyster is a case of two parallel diseases, comparable to the case of a dog infected simultaneously with tapeworms and mange. According to Jameson, pearls arise round nuclei of some variety of shell-substance formed when the normal rhythm of secretion is disturbed.

SOLAR DISTURBANCES DURING AUGUST, 1912.

By FRANK C. DENNETT.

AUGUST was remarkable for its lack of clear sky, but during the month only two days were entirely missed, the 15th and the 23rd. Spots were only seen on four days, the 11th, 12th, 21st and 28th, and on ten others only faculae were visible, leaving fifteen upon which the disc appeared quite free from disturbance. At noon on August the 1st the longitude of the central meridian was 224° 42′.

No. 12.—A minute group of pores only seen on the evening of the 11th and on the morning of the 12th, the true position having to be estimated rather than measured.

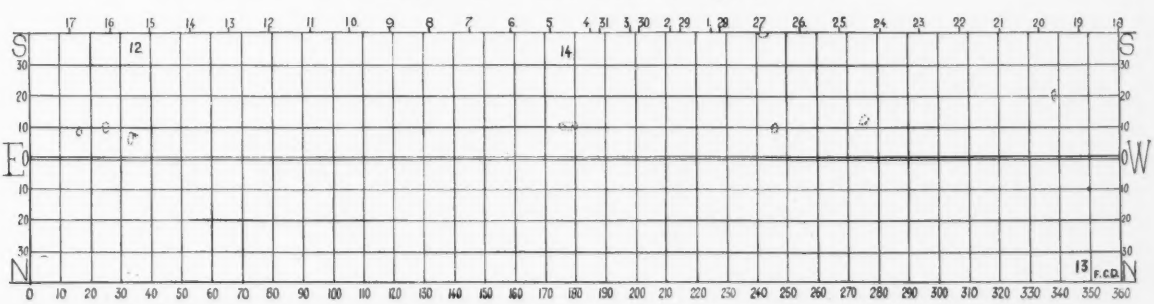
No. 13.—A penumbraless pore, bright edged, with its eastern edge somewhat rough, and on its western side the surface showed traces of minute black specks, but it only lasted one day, the 21st.

No. 14.—A group of minute pores observed with difficulty on the 28th in the place seen on the previous day to be occupied by a faculic area. The position has been given by estimation, as measures were impossible. It appears to have been situated directly south of the place in which No. 11 had been seen in July.

Faculic areas were observed on August 2nd and 3rd about longitude 242°, and south latitude 40°. On the 7th, near the western limb. On the 10th and 11th east toward south-east, in longitudes 16° and 34° and latitudes 9° and 4° to 8° south respectively. On the 19th, within the eastern limb a little south, in longitude 276° and latitude 12°. On the 21st a faculic ridge from 9° to 11° south, near the western limb, longitude 25°; also a knot at 247°, 10° south, and another in very high southern latitudes near the central meridian. On the 25th a double knot of faculae was approaching the south-western limb, situated longitude 339°, and south latitude 19° to 22°. And on the 27th, some 30° within the eastern limb, longitude 179°, south latitude 10°, the faculic disturbance was seen which heralded the group No. 14.

Our chart is constructed from the combined observations of Messrs. J. McHarg, A. A. Buss, C. Frooms, and the writer.

DAY OF AUGUST, 1912.



XUM

SIR JOSEPH DALTON HOOKER,

O.M., G.C.S.I., C.B., M.D., F.R.S. (1817–1911).

By F. O. BOWER, Sc.D., F.R.S.,

Regius Professor of Botany in the University of Glasgow.

(Being the Chairman's Address to the Conference of Delegates at the Dundee Meeting of the British Association for the Advancement of Science, 1912.)

I HAVE thought that in addressing this conference of delegates, I could not do better than take as my subject Sir Joseph Dalton Hooker, whose death in December, 1911, has closed a strenuous life of ninety-four years. The life we now commemorate was one of action throughout. Naturally with age the bodily strength waned. But Sir Joseph Hooker's vivid mind remained unimpaired to the end. He even continued his detailed observations till very shortly before his death in December last. The list of his published works extends from 1807 to 1911, a record hardly to be equalled in any walk of intellectual life.

I do not propose to give any consecutive biographical sketch of this great man. Several such have already appeared. I think I shall better engage your interest by indicating the various lines of activity in which he excelled. He was never a professional teacher, except for a short period of service as assistant in Edinburgh. There was a moment when he might have been Professor in Edinburgh, but it passed. He left no pupils, except in the sense that all botanists have learned from him through his books. We shall contemplate him rather as a traveller and geographer, as a geologist, as a morphologist, as an administrator, as a scientific systematist, and above all as a philosophical biologist. He was all of these, and almost any of these heads might have sufficed for a complete address. I will endeavour, however imperfectly, to touch upon them all.

The experiences of Hooker as a *traveller* began immediately after taking his degree, with his commission in 1839 as assistant surgeon and botanist in the "Erebus." Scientific Exploration was still in its heroic age. Darwin was only three years back from the voyage of the "Beagle." We may well hold the years from 1831, when the "Beagle" sailed, to 1851, when Hooker returned from his Indian journey, or 1852, when Wallace returned from the Amazon, to have been its golden period. Certainly it was if we measure by results. Unmatched opportunity for travel in remote and unknown lands was then combined with unmatched capacity of those who engaged in it. Nor was this a mere matter of chance. For Darwin, Wallace, and Hooker all seized, if they did not in some measure make, their opportunity.

The intrepid Ross, with his two sailing ships, the "Erebus" and the "Terror," probed at suitable seasons during four years the extreme south. The very names of the Great Ice Barrier, M'Murdo Sound, Mount Erebus and Mount Terror, made familiar to us by adventures seventy years later under steam, remain to mark some of his additions to the map of the world. Young Hooker took his full share of risks, up to the point of being peremptorily ordered back on one occasion by his commanding officer. To his activity and willingness, combined with an opportunity that can never recur in the same form, is due that great collection of specimens, and that wide body of fact which he acquired. On the outward and return voyages, or in the intervals when the season was not favourable for entering the extreme southern seas, the expedition visited Ascension, St. Helena, the Cape, New Zealand, Australia, Tasmania, Kerguelen Island, Tierra del Fuego, and the Falkland Islands. The prime object of the voyage was a magnetic survey, and this determined its course. But it brought this secondary consequence; that Hooker had the chance of observing and collecting upon all the great circumpolar areas of the southern hemisphere. The results he later welded together into his first great work, "The Antarctic Flora."

Very soon after his return from the Antarctic the craving for travel broke out afresh in him. He longed to see a tropical Flora in a mountainous country, and to compare it at different levels with that of temperate and arctic zones. Two alternatives arose before him: the Andes and the Himalaya. He chose the latter, being influenced by promises of assistance from Dr. Falconer, the Superintendent of the Calcutta Garden. But before he left England his journey came under the recognition of Government. He not only received grants on the condition that the collections made should be located in the Herbarium at Kew, but he was accredited by the Indian Government to the Rulers, and the British Residents, in the countries whose hitherto untrodden ways he was to explore. After passing the cold season of 1848 in making himself acquainted with the vegetation of the plains and hills of Western Bengal, he struck north to the Sikkim Himalaya. Hither he had been directed by Lord Auckland and by Dr. Falconer, as to ground unbroken by traveller or naturalist. The story of this remarkable journey, its results and its vicissitudes, including the forcible detention of himself and his companion, Dr. Campbell, by a faction of the Court of Sikkim, is to be found in his "Himalayan Journal." This most fascinating volume of travel was published in 1854. It tells how he spent two years in the botanical exploration and topographical survey of the state of Sikkim, and of a number of the passes leading into Thibet; and how towards the close of 1848 he even crossed the western frontier of Sikkim, and explored a portion of Nepal that has never since been open to travellers. In 1849 he returned to Darjeeling, and busied himself with arranging his vast collections. Here he was joined by an old fellow-student of Glasgow, Dr. Thomas Thomson, son of the professor of that name. The two friends spent the year 1850 in the botanical investigation of Eastern Bengal, Chittagong, Silhet, and the Khasia hills. In 1851 they returned together to England.

The botanical results of these Indian journeys were immense, and they provided the material for much of Hooker's later scientific writing. Nearly seven thousand species of Indian plants were collected by these two Glasgow graduates. But Hooker was not a mere specialist. His Journal is full of other observations, ethnographical, ornithological and entomological. His topographical results especially were of the highest importance. They formed the basis of a map published by the Indian Topographical Survey. By the aid of it the operations of various campaigns and political missions have since been carried to a successful issue. If he were not known as a botanist, he would still have his assured place as a geographer.

After his return from India, nine years ensued of quiet work at home. But in 1860 Hooker took part in a scientific visit to Syria and Palestine, ascending Mount Lebanon, where he specially paid attention to the decadent condition of the Cedars, his observations leading later to a general discussion of the genus. Again a period of ten years intervened, his next objective being Morocco. In 1871, with Mr. Ball and Mr. Maw, he penetrated the Atlas Range, never before examined botanically. His last great journey was in 1877, when he was sixty years of age. With his old friend, Prof. Asa Gray, of Harvard, he visited Colorado, Wyoming, Utah, the Rocky Mountains, the Sierra Nevada, and California. Prof. Coulter, of Chicago, who was one of the party in the Rockies, has told me how difficult it was to round up the two elderly enthusiasts to camp at night.

This is an extraordinary record of travel, especially so when we remember that all the journeys were fitted into the intervals

XUM

of an otherwise busy life of scientific work and administration. At one time or another he had touched upon every great continental area of the earth's surface. Many isolated islands had also been examined by him, especially on the Antarctic voyage. Not only were fresh regions thus opened up for survey and collection, but each objective of the later journeys was definitely chosen for scientific reasons. Each expedition helped to suggest or to solve major problems. Such problems related not only to the distribution, but also to the very origin of species. Darwin saw this with unerring judgment as early as 1845. Hooker was then but twenty-eight years old, and the records of the Antarctic voyage were only in preparation. Nevertheless, Darwin wrote with full assurance in a letter to Hooker himself: "I know I shall live to see you the first authority in Europe on that grand subject, that almost keystone of the laws of Creation, Geographical Distribution." Never was a forecast more fully justified. But that position, which Hooker undoubtedly had, could only have been attained through his personal experience as a traveller. Observation at first hand was the foundation upon which he chiefly worked. Hooker the traveller prepared the way for Hooker the philosopher.

Sir Joseph Hooker would probably have declined to consider himself as a *geologist*. He was, however, for some eighteen months official botanist to the Geological Survey of Great Britain. He was appointed in April, 1846, but relinquished the post in November, 1847, in order to start on his Himalayan journey. During that short period three memoirs were published by him on plants of the Coal Period. They embodied results derived from the microscopic examination of plant-tissues preserved in Coal Balls, a study then newly introduced by Witham, and advanced by Mr. Binney. It has since been greatly developed in this country. Such studies were continued by him at intervals up to 1855. While he was thus among the first to engage in this branch of enquiry, he may be said to have originated another line of study, since largely pursued by geologists. For he examined samples of diatomaceous ooze from the ocean-floor of the Antarctic, and so initiated the systematic treatment of the organic deposits of the deep sea. Yet another branch of geological enquiry was advanced by him in the Himalaya. For there he made observations on the glaciers of that great mountain chain, his notes supplying valuable material to both Lyell and Darwin. He also accumulated valuable data concerning the stupendous effects of sub-aërial denudation at great elevations. His latest contribution of a geological character was in 1889, when he returned to an old problem of his youth, the Silurian fossil, *Pachytheca*. But he had to leave the question of its nature still unsolved. This geological record is not an extensive one. But the quality and rapidity of the work showed that it was the time and opportunity and not the faculties that were wanting. Moreover, it is worthy of remark that the problems he handled were all nascent at the time he worked upon them.

The list of Sir Joseph Hooker's memoirs which deal *morphologically* with more limited subjects than is possible in floristic works, is a restricted one. In 1856 he produced a monograph on the Balanophoraceae, based upon collections of material from the most varied sources. It is still an authority very widely quoted on these strange parasites. In 1859 he described the development and structure of the Pitchers of *Nepenthes*, while the physiological significance of these and other organs of carnivorous plants, formed the subject of an Address before the British Association at Belfast, in 1874. And in 1863 his great monograph appeared upon that most remarkable of all Gymnospermic plants, *Welwitschia*. These works bore the character of a later period than the time when they were produced. In Britain, between 1840 and 1875, investigation in the laboratory, by microscopic analysis of tissues, was almost throttled by the overwhelming success of systematic and descriptive work. The revival of investigation in the laboratory rather than that in the herbarium dates from about 1875. But we see that Hooker was one of the few who, prior to that revival, pursued careful microscopic analysis side by side with systematic and floristic work.

The noble establishment of the Royal Gardens at Kew is often spoken of as the Mecca of botanists. It is also the Paradise of the populace of London. It was the Hookers, father and son, who made Kew what it is. When we contemplate Sir Joseph as an *administrator*, we immediately think of the great establishment which he and his father ruled during the first half century of its history as a public institution. Kew had existed for long as a Royal appanage before it was handed over to the nation. The Botanic Garden had, indeed, ranked for upwards of half a century as the richest in the world. But after the death of King George III it had retrograded scientifically. On the accession of Queen Victoria a revision of the Royal Household had become necessary. It was then decided to transfer the garden to the Commissioners of Woods and Forests. This took place in 1840, and in 1841 Sir William Hooker, who was then Professor in Glasgow, was appointed the first Director. The move to Kew, whither he took his private library, herbarium, and museum, was carried out in the absence of his son, who was still in the Antarctic. It was not till the Himalayan journey was over in 1851 that Sir Joseph settled at Kew, his great collections having already been consigned there by agreement with the Government. In 1855 he was appointed assistant to his father in the Directorship. Finally, he became himself Director on his father's death in 1865, and he held the position for twenty years.

So long associated together, it is difficult to disentangle the parts that father and son actually played in the creation of Kew as it now is. Nor is there need to attempt it. The original area of the Garden at Kew was less than twenty acres. But in 1855, when Sir Joseph joined his father in the directorate, it had grown by successive additions to seventy acres. Finally, the large area of six hundred and fifty acres came under the Director's control. Numerous large glass houses were built. Three museums were established, and the vast Herbarium and Library founded and developed. The Garden Staff rose to more than one hundred men. The day-by-day administration of such an establishment would necessarily make great demands upon the time, energy, tact, and skill of its official head. But in addition there was the growing correspondence to be attended to, on the one hand with botanists all over the world, on the other with the Government Departments, and especially with the Indian and Colonial Offices. As the activity of the Garden extended, there grew up a large staff of scientific experts and artists, whose duties centred round the Herbarium and Library. These all looked to the Director for their guidance and control. The descriptive work prepared by them for publication took formidable dimensions. The production of the floras of India, and of the Colonies, the publication of which was conducted under Government subvention, had to be organised and carried through. These matters are mentioned here to give you some idea of the extent and complexity of the work which was being carried on at Kew. For ten years as Assistant Director, and for twenty years as Director, Sir Joseph Hooker guided this complex machine. The efficiency of his rule was shown by the increasing estimation in which the Garden was held by all who were able to judge.

It was the founding of the Herbarium and Library at Kew which, more than anything else, strengthened the scientific establishment. As taken over from the Crown the Garden possessed neither. But Sir William brought with him from Glasgow his own collections, already the most extensive in private hands. For long years after coming to Kew he maintained and added to his store at his own expense. But finally his collections were acquired after his death by Government. His herbarium was merged with the fine herbarium of Bentham, already presented to the nation in 1857. Thus, the opening years of Sir Joseph's directorate saw the organisation upon a public basis of that magnificent Herbarium and Library, which now contains not only his father's collections, but also his own. Among the enormous additions since made to the Herbarium of Kew, its greatest interest will always be centred in the Hookerian collections which it contains.

It might be thought that such drafts as these upon the time and energies of a scientific man would leave no opportunity for other duties. But it was while burdened

XUM

with the directorship that Sir Joseph was called to the highest administrative office in science in Great Britain. He served as President of the Royal Society from 1873 to 1878. The obligations of that position are far from being limited to the requirements of the Society itself. The Government of the day has always been in the habit of taking its president and officials into consultation in scientific matters of public importance. In these years the administrative demands upon Sir Joseph were the greatest of his life. They are marked by a temporary pause in the stream of publication. None of his own larger works belong to this period. It happens only too often in this country that our ablest men are thus paralysed in their scientific careers by the potent vortex of administration. Not a few succumb, and cease altogether to produce. They are caught as in the eddy of the Lorelei, and are so hopelessly entangled that they never emerge again. They fail to realise, or realise too late, that the administration of matters relating to a science is not an end in itself, but only a means to an end. Some, the steadfast and invincible seekers after truth, though held by the eddy for a time, pass again into the main stream. Hooker was one of these. The Presidency of the Royal Society ended at the usual term of five years. Seven years later he demitted office as Director of Kew, under the Civil Service rule. He was thus free in 1885, still a young man in vigour though not in years. For over a quarter of a century after retirement he devoted the energy of his old age to peculiarly fruitful scientific work. Thus the administrative tie upon him was only temporary. So long as it lasted he faithfully obeyed the call of duty, notwithstanding the restrictions it imposed.

I shall not attempt to give an exhaustive catalogue of the works upon which the reputation of Sir Joseph Hooker as a *scientific systematist* was founded. Were I to do so our time would be gone before the list was completed. It must suffice briefly to consider his four greatest systematic works, "The Antarctic Flora," "The Flora of British India," "The Genera Plantarum," and the "Kew Index."

We have seen how on the Antarctic voyage Hooker had the opportunity of collecting on all the great circumpolar areas of the Southern Hemisphere. His "Antarctic Flora" was based on the collections and observations then made. It was published in six large quarto volumes. All the known facts that could be gathered were incorporated, so that they became systematically elaborated and complete floras of the several countries. Moreover, in the last of them, the "Flora Tasmaniae," there is an Introductory Essay, which in itself would have made Hooker famous. We shall return to this later. Meanwhile we recognise that the publication of the "Botanical Results of Ross's Voyage" established Hooker's reputation as a traveller and botanist of the first rank.

What he did for the Antarctic in his youth he continued in mature life for British India. While the publication of the "Antarctic Flora" was still in progress, he made his Indian journeys. The vast collections amassed by himself and Dr. Thomson were consigned by agreement with Government to Kew. Thither had also been brought in 1858 "seven waggon-loads of collections from the cellars of the India House in Leadenhall Street, where they had been accumulating for many years." They included the herbaria of Falconer and Griffith. Such materials, with other large additions made from time to time, flowed into the already rich Herbarium at Kew. This was the material upon which Sir Joseph Hooker was to base his *Magnum Opus*, the "Flora of British India."

Already in 1855 Sir Joseph, with his Glasgow college friend, Thomas Thomson, had essayed to prepare a "Flora Indica." It never advanced beyond its first volume. But if it had been completed on the scale set by that volume, it would have reached nearly twelve thousand pages! After a pause of over fifteen years Hooker made a fresh start, aided now by a staff of collaborators, and the "Flora of British India" was the result. It was conceived, he says with regret, upon a restricted plan. Nevertheless it ran to seven volumes, published between the years 1872 and 1897. There are nearly six thousand pages of letterpress, relating to sixteen thousand species. It is, he says in the Preface, a pioneer work, and necessarily incomplete. But he hopes it may "help the phytographer to discuss problems of distribution of plants from the point of view of what is perhaps the richest, and is certainly the most varied botanical area on the surface of the globe."

Scarcely was this great work ended when Dr. Trimen died. He left the "Ceylon Flora," on which he had been engaged, incomplete. Three volumes were already published, but the fourth was far from finished, and the fifth hardly touched. The Ceylon Government applied to Hooker, and though he was now eighty years of age, he responded to the call. The completing volumes were issued in 1898 and 1900. This was no mere raking over afresh the materials worked already into the "Indian Flora." For Ceylon includes a strong Malayan element in its vegetation. It has, moreover, a very large number of endemic species, and even genera. This last floristic work of Sir Joseph may be held fitly to round off his treatment of the Indian Peninsula. His last contribution to its botany was in the form of a "Sketch of the Vegetation of the Indian Empire," including Ceylon, Burma, and the Malay Peninsula. It was written for the "Imperial Gazetteer," at the request of the Government of India. No one could have been so well qualified for this as the veteran who had spent more than half a century in preparation for it. It was published in 1904, and forms the natural close to the most remarkable study of a vast and varied flora that has ever been carried through by one ruling mind.

The third of the systematic works selected for our consideration is the "Genera Plantarum." It was produced in collaboration with Mr. Bentham. Of its three massive volumes the first was published in 1865, and the work was completed in 1883. It consists of a codification of the Latin diagnoses of all the genera of flowering plants. It is essentially a work for the technical botanist, but for him it is indispensable. Of the known species of plants many show such close similarity of their characters that their kinship is recognised by grouping them into genera. In order that these genera may be accurately defined it is necessary to have a *précis* of the characters which their species have in common. This must be so drawn that it shall also serve for purposes of diagnosis from allied genera. Such drafting requires not only a keen appreciation of fact, but also the verbal clearness and accuracy of the conveyancing barrister. The facts could only be obtained by access to a reliable and rich herbarium. Bentham and Hooker, working together at Kew, satisfied these drastic requirements more fully than any botanists of their time. The only real predecessors of this monumental work were the *Genera Plantarum* of Linnaeus (1707-1764) and of Jussieu (1789), to which may be added that of Endlicher (1836-1840). But all of these were written while the number of known genera and species was smaller. The difficulty of the task of Bentham and Hooker was greatly enhanced by their wider knowledge. But their *Genera Plantarum* is on that account a nearer approach to finality. Hitherto its supremacy has not been challenged.

The fourth of the great systematic works of Hooker mentioned above was the *Index Kewensis*. It was produced upon the plan and under the supervision of Sir Joseph by Mr. Daydon Jackson and a staff of clerks. The publication began in 1893, and successive supplements to its four quarto volumes are still appearing at intervals. The expense was borne by Charles Darwin. The scheme originated in the difficulty he had found in the accurate naming of plants. For "synonyms" have frequently been given by different writers to the same species, and this had led to endless confusion. The object of the Index was to provide an authoritative list of all the names that have been used, with reference to the author of each and to its place of publication. The habitat of the plant was also to be given. The correct name in use according to certain well-recognised rules of nomenclature was to be indicated by type different from that of the synonyms superseded by it. The only predecessor of such an Index was Steudel's *Nomenclator Botanicus*, a book greatly prized by Darwin, though long out of date. He wished at first to produce a modern edition of Steudel's *Nomenclator*. This idea was,

however, amended, and it was resolved to construct a new list of genera and species, founded upon Bentham and Hooker's *Genera Plantarum*. Sir Joseph Hooker was asked by Mr. Darwin to take into consideration the extent and scope of the proposed work, and to suggest the best means of having it executed. He undertook the task, and it was he who laid out the lines to be followed. After years of labour by Mr. Daydon Jackson and his staff, the work was produced. But Sir Joseph read and narrowly criticised all the proofs. Imagine four large quarto volumes, containing in the aggregate two thousand five hundred pages, each page bearing three columns of close print, and each column about fifty names. The total figures out to about three hundred and seventy-five thousand specific names, all of which were critically considered by the octogenarian editor! Surely no greater technical benefit was ever conferred upon a future generation by the veterans of science than this Index. It smooths the way for every systematist who comes after. It stands as a monument to an intimate friendship. It bears witness to the munificence of Darwin, and the ungrudging personal care of Hooker.

But the author of great works such as these was still willing to help those of less ambitious flights. I must not omit to mention two books which, being more modest in their scope, have reached the hands of many in this country. In 1870 Hooker produced his "Student's Flora of the British Islands," of which later editions appeared in 1878 and 1884. It was published in order to "supply students and field botanists with a fuller account of the plants of the British Isles than the manuals hitherto in use aim at giving." In 1887 he edited, after the death of its author, the fifth edition of Bentham's "Handbook of the British Flora." Both of these still hold the field, though they require to be brought up to date in point of classification and nomenclature.

I hope I have not wearied you with these brief sketches of four of the great systematic works of Sir Joseph Hooker. I have gone somewhat more into detail than is quite justified in a public speech. But this has been done with a definite end in view. It was to show you how fully he was imbued with the old systematic methods: how he advanced, improved and extended them, and was in his time their chief exponent. His father had held a similar position in the generation before him. But the elder Hooker, true to his generation, treated his species as fixed and immutable. He did not generalise from them. His end was attained by their accurate recognition, delineation, description, and classification. The younger Hooker, while in this work he was not a whit behind the best of his predecessors, saw further than they. He was not satisfied with the mere record of species as they were. He sought to penetrate the mystery of the origin of species. In fact, he was not merely a scientific systematist in the older sense. He was a ***philosophical biologist*** in the new and nascent sense of the middle period of the nineteenth century. He was an almost life-long friend of Charles Darwin. He was the first confidant of his species theory, and, excepting Wallace, its first whole-hearted adherent. But he was also Darwin's constant and welcome adviser and critic. Well indeed was it for the successful launch of evolutionary theory that old-fashioned systematists took it in hand. Both Darwin and Hooker had wide and detailed knowledge of species as the starting-point of their induction.

Before we trace the part which Hooker himself played in the drama of evolutionary theory, it will be well to glance at his personal relations with Darwin himself. He had read the proof-sheets of the "Voyage of the 'Beagle'" while still in his last year of medical study. But before he started for the Antarctic he was introduced to its author. It was in Trafalgar Square, and the interview was brief but cordial. On returning from the Antarctic, correspondence was opened in 1843. In January, 1844, Hooker received the memorable letter confiding to him the germ of the Theory of Descent. Darwin wrote thus: "At last gleams of light have come, and I am almost convinced that species are not (it is like confessing a murder) immutable:—I think I have found (here's presumption!) the simple way by which species become exquisitely adapted to various ends." This was probably the first communication by Darwin of his species-theory to any scientific colleague.

The correspondence thus happily initiated between Darwin and Hooker is preserved in the "Life and Letters of Charles Darwin," and in the two volumes of "Letters" subsequently published. They show on the one hand the rapid growth of a deep friendship between these two potent minds, which ended only beside the grave of Darwin in Westminster Abbey. But what is more important is that these letters reveal, in a way that none of the published work of either could have done, the steps in the growth of the great generalisation. We read of the doubts of one or the other; the gradual accumulation of material facts; the criticisms and amendments in face of new evidence; and the slow progress from tentative hypothesis to assured belief. We ourselves have grown up since the clash of opinion for and against the mutability of species died down. It is hard for us to understand the strength of the feelings aroused: the bitterness of the attack by the opponents of the theory, and the fortitude demanded from its adherents. It is best to obtain evidence on such matters at first hand; and this is what is supplied by the correspondence between Darwin and Hooker.

How complete the understanding between the friends soon became is shown by the provisions made by Darwin for the publication of his manuscripts in case of sudden death. He wrote in August, 1854, the definite direction "Hooker by far the best man to edit my species volume": and this notwithstanding that he writes to him as a "stern and awful judge and sceptic." But again, in a letter a few months later, he says to him: "I forgot at the moment that you are the one living soul from whom I have constantly received sympathy." I have already said that Hooker was not only Darwin's first confidant but also the first to accept his theory of mutability of species. But even he did not fully assent to it till after its first publication. The latter point comes out clearly from the letters. In January, 1859, six months after the reading of their joint communications to the Linnaean Society, Darwin writes to Wallace: "You ask about Lyell's frame of mind. I think he is somewhat staggered, but does not give in . . . I think he will end by being perverted. Dr. Hooker has become almost as heterodox as you or I, and I look at Hooker as by far the most capable judge in Europe." In September 1859 Darwin writes to W. D. Fox: "Lyell has read about half of the volume in clean sheets . . . He is wavering so much about the immutability of species that I expect he will come round. Hooker has come round, and will publish his belief soon." In the following month, writing to Hooker, Darwin says: "I have spoken of you here as a convert made by me: but I know well how much larger the share has been of your own self-thought." A letter to Wallace of November, 1859, bears this postscript: "I think that I told you before that Hooker is a complete convert. If I can convert Huxley I shall be content." And lastly, in a letter to W. B. Carpenter, of the same month, Darwin says: "As yet I know only one believer, but I look at him as of the greatest authority, viz., Hooker." These quotations clearly show that, while Lyell wavered, and Huxley had not yet come in, Hooker was a complete adherent in 1859 to the doctrine of the mutability of species. Excepting Wallace, he was the first, in fact, of the great group that stood round Darwin, as he was the last of them to survive.

The story of the joint communication of Darwin and of Wallace to the Linnaean Society "On the tendency of Species to form Varieties, and on the Perpetuation of Varieties and Species by Natural Means of Selection" will be fresh in the minds of you all, for the fiftieth anniversary of the event was lately celebrated in London. It was Sir Charles Lyell and Sir Joseph Hooker who jointly, and with the authors' permission, communicated the two papers to the society, together with the evidence of the priority of Darwin in the enquiry. Nothing could then have been more apposite than the personal history which Sir Joseph gave at the Darwin-Wallace celebration, held by the Linnaean Society in 1908. He then told, at first hand, the exact circumstances under which the joint papers were produced. Nor could the expressions used by the President when thanking

XUM

Sir Joseph, and presenting to him the Darwin-Wallace Medal, have been improved. He said: "The incalculable benefit that your constant friendship, advice, and alliance were to Mr. Darwin himself, is summed up in his own words, used in 1864: 'You have represented for many years the whole great public to me.'" The President then added: "Of all men living it is to you more than to any other that the great generalisation of Darwin and Wallace owes its triumph."

The very last appearance of Hooker at any large public gathering of biologists was at the centenary of Darwin's birth, celebrated at Cambridge, in 1909. None who were there will forget the tall figure of the veteran, aged, but still vigorous, with vivacity in every feature. How gladly he accepted the congratulations of his many friends, and how heartily he rejoiced over the full acceptance of the theory he had himself done so much to promote. The end came only two years later, in December last. Many will have wished that the great group of the protagonists of Evolution, Darwin, Lyell, and Hooker, should have found their final resting-place together in Westminster Abbey. But this was not to be. Personal and family ties held him closer to Kew. And he lies there in classic ground beside his father.

Having thus sketched the intimate relations which subsisted between Hooker and Darwin, it remains to appraise his own positive contributions to Philosophical Biology. He himself, in his Address as President of the British Association at Norwich in 1868, gives an insight into his early attitude in the enquiry into biological questions. "Having myself," he says, "been a student of Moral Philosophy in a Northern University, I entered on my scientific career full of hopes that Metaphysics would prove a useful mentor, if not a guide in science. I soon found, however, that it availed me nothing, and I long ago arrived at the conclusion so well put by Agassiz, when he says: 'We trust that the time is not distant when it will be universally understood that the battle of the evidences will have to be fought on the field of Physical Science, and not on that of the Metaphysical." This was the difficult lesson of the period when Evolution was born. Hooker learned the lesson early. He cleared his mental outlook from all preconceptions, and worked down to the bed-rock of objective fact. Thus he was free to use his vast and detailed knowledge in advancing, along the lines of induction alone, towards sound generalisations. These had their very close relation to questions of the mutability of species. The subject was approached by him through the study of geographical distribution, in which, as we have seen, he had at an early age become the leading authority.

The fame of Sir Joseph Hooker as a philosophical biologist rests upon a masterly series of Essays and Addresses. The chief of these were "The Introductory Essay to the *Flora Tasmaniae*, dealing with the Antarctic Flora as a whole"; "The Essay on the Distribution of Arctic Plants," published in 1862; "The Discourse on Insular Floras," in 1866; The Presidential Address to the British Association at Norwich in 1868; his Address at York, in 1881, on Geographical Distribution; and finally, "The Essay on the Vegetation of India," published in 1904. None of these were mere inspirations of the moment. They were the outcome of arduous journeys to observe and to collect, and subsequently of careful analysis of the specimens and of the facts. The dates of publication bear this out. "The Essay on the Antarctic Flora" appeared about twenty years after the completion of the voyage. "The Essay on the Vegetation of India was not published till more than half-a-century after Hooker first set foot in India. It is upon such foundations that Hooker's reputation as a great constructive thinker is based.

The first-named of these essays will probably be estimated as the most notable of them all in the history of Science. It was completed in November, 1859, barely a year after the joint communications of Darwin and Wallace to the Linnaean Society, and before the "Origin of Species" had appeared. It was to this Essay that Darwin referred, when he wrote that "Hooker has come round, and will publish his belief soon." But this publication of his belief was not merely an echo of assent of Darwin's own opinions. It was a reasoned statement, advanced upon the basis of his "own self-thought," and his own wide systematic and geographical experience. From these sources he drew for himself support for the "hypothesis that species are derivative, and mutable." He points out how the natural history of Australia seemed specially suited to test such a theory, on account of the comparative uniformity of the physical features being accompanied by a great variety in its flora, and the peculiarity of both its fauna and flora, as compared with other countries. After the test had been made, on the basis of study of some 8000 species, their characters, their spread, and their relations to other lands, he concludes decisively in favour of mutability and a doctrine of progression.

How highly this Essay was esteemed by his contemporaries is shown by the expressions of Lyell and of Darwin. The former writes: "I have just finished the reading of your splendid Essay on the Origin of Species, as illustrated by your wide botanical experience, and think it goes far to raise the variety-making hypothesis to the rank of a theory, as accounting for the manner in which new species enter the world." Darwin wrote: "I have finished your Essay. To my judgment it is by far the grandest and most interesting essay on subjects of the nature discussed I have ever read."

But besides its historical interest in relation to the species question, the Essay contained what was up to its time the most scientific treatment of a large area from the point of view of the plant-geographer. He found that the Antarctic, like the Arctic, Flora is very uniform round the Globe. The same species in many cases occur on every island, though thousands of miles of ocean may intervene. Many of these species reappear on the mountains of Southern Chili, Australia, Tasmania. and New Zealand. The Southern Temperate Floras, on the other hand, of South America, South Africa, Australia, and New Zealand differ more among themselves than do the Floras of Europe, Northern Asia and North America. To explain these facts he suggested the probable former existence, during a warmer period than the present, of a centre of creation of new species in the Southern Ocean, in the form of either a continent or an archipelago, from which the Antarctic Flora radiated. This hypothesis has since been held open to doubt. But the fact that it was suggested shows the broad view which he was prepared to take of the problem before him. His method was essentially that which is now styled "Ecological." Many hold this to be a new phase of botanical enquiry, introduced by Professor Warming in 1895. No one will deny the value of the increased precision which he then brought into such studies. But in point of fact it was ecology on the grand scale that Sir Joseph Hooker practised in the Antarctic in 1840. Moreover, it was pursued, not in regions of old civilisation, but in lands where Nature held her sway untouched by the hand of man.

This Essay on the Flora of the Antarctic was the prototype of the great series. Sir Joseph examined the Arctic Flora from similar points of view. He explained the circumpolar uniformity which it shows, and the prevalence of Scandinavian types, together with the peculiarly limited nature of the Flora of the southward peninsula of Greenland. He extended his enquiries to oceanic islands. He pointed out that the conditions which dictated circumpolar distribution are absent from them; but that other conditions exist in them which account for the strange features which their vegetation shows. He extended the application of such methods to the Himalaya and to Central Asia. He joined with Asa Gray in like enquiries in North America. The latter had already given a scientific explanation of the surprising fact that the plants of the Eastern States resemble more nearly those of China than do those of the Pacific Slope. In resolving these and other problems it was not only the vegetation itself that was studied. The changes of climate in geological time, and of the earth's crust as demonstrated by geologists, formed part of the basis on which he worked. For it is facts such as these which have determined the migration of floras. And migration, as well as mutability of species, entered into most of his speculations. The essays of

this magnificent series are like pictures painted with a full brush. The boldness and mastery which they show sprang from long discipline and wide experience.

Finally, the chief results of the phyto-geographical work of himself and of others were summed up in the great address on "Geographical Distribution" at York. The Jubilee of the British Association was held there in 1881. It had been decided that each section should be presided over by a past President of the Association, and he had occupied that position at Norwich in 1868. Accordingly, at York Hooker was appointed President of the Geographical Section, and he chose as the subject of his address "The Geographical Distribution of Organic Beings." To him it illustrated "the interdependence of those Sciences which the Geographer should study." It is not enough merely to observe the topography of organisms, but their hypsometrical distribution must also be noted. Further, the changes of area and of altitude is exposed land-surfaces of which geology gives evidence, are essential features in the problem, together with the changes of climate, such as have determined the advance and retrocession of glacial conditions. Having noted these factors, he continued thus: "With the establishment of the doctrine of orderly evolution of species under known laws I close this list of those recognised principles of the science of geographical distribution, which must guide all who enter upon its pursuit. As Humboldt was its founder, and Forbes its reformer, so we must regard Darwin as its latest and greatest law-giver." Now, after thirty years, may we not add these words of his, that Hooker was himself its greatest exponent?

And so we have followed, however inadequately, this famous man into the various lines of scientific activity which he pursued. We have seen him to excel in them all. The cumulative result is that he is universally held to have been, during several decades, the most distinguished botanist of his time.

He was before all a philosopher. In him we see the foremost student of the broader aspects of plant-life at the time when evolutionary belief was nascent. His influence at that stirring period, though quiet, was far-reaching and deep. His work was both critical and constructive. His wide knowledge, his keen insight, his fearless judgment were invaluable in advancing that intellectual revolution which found its pivot in the mutability of species. The share he took in promoting it was second only to that of his life-long friend, Charles Darwin.

THE ROMANCE OF MATHEMATICS.

By F. T. DEL MARMOL, B.Sc. (Paris), C.E. (Madrid).

CAMBRIDGE, with its glorious scientific history, has had under the hospitable roof of its University the fifth international congress of mathematics, presided over by Sir George Darwin, and attended by many eminent mathematicians of the world.

The most interesting of all the speeches has been the presidential address, the culminating point of which was the distinction established by Sir George between pure and applied mathematics, showing clearly the superiority of the former, which never err in their results, while deductions made from their applications to other sciences must depend upon the greater or lesser exactness of the hypotheses on which the applications are based. As an example the chairman quoted the marvellous and complex calculations of Lord Kelvin, who conceded a superior limit of less than one hundred million years as the age of the solid mass of our globe, while geologists, after observing the different terrestrial strata, insisted, and still do so, on a minimum of eight hundred million years. At the present time the conclusions of Kelvin have been generally rejected. Professor Strutt has demonstrated conclusively, from a study of the properties of radio-active bodies, that this limit must be at least seven hundred and eleven million years, a figure which agrees fairly well with that put forward by the geologists. The error of the great mathematician may be easily explained: firstly, he was dealing with a problematic ground sown thickly with hypotheses and more or less contradictory facts; secondly, when Lord Kelvin made his calculations, the extremely modern theories of radio-activity were unknown. But it would be unjust to put the blame on the science of mathematics for this or for similarly unsatisfactory results. The calculations of that great man, whom many called the second Newton, were always marvellous. But as in this particular case he had to work out the problem with incomplete data, the results were necessarily defective.

Many similar cases might be cited. There have been few mathematicians worthy of being compared with Laplace and Lagrange, who imagined they had proved the absolute stability of the solar system, when there is no such stability. The calculations of the two great Frenchmen remain a model of elegance and rigorous precision. Unfortunately, they started from false premises: they supposed that all the bodies of the system possess an absolute rigidity, whereas there is no star absolutely rigid. If they were so, then Lagrange's famous prophecy regarding the eternal duration of the solar system would be true. Rightly did the chairman of the Congress point out that pure mathematics must not be confused with applied, since, although both aim at truth, the former seeks absolute truth, while the latter essays, often without success, to discover truths about the universe! Sir George Darwin himself has presented astronomers with a most fascinating mathematical theory of the Moon as born from the Earth, and notwithstanding the rigorous reasoning and the severely accurate mathematics of this illustrious astronomer, it is obvious that his theory can only be true if our planet possessed once the stupendous rotational velocity that Sir George supposes it to have possessed at a certain epoch.

A Frenchman over whose recent death Science mourns to-day, Henri Poincaré, who was the world's first mathematician—with the exception, I believe, of Sir George Darwin, and to whose last works many eulogistic references were made at the congress of Cambridge, succeeded, before his death, in crowning his already gigantic labour by the publication of a powerful work, "Les Hypothèses Cosmogoniques," in which he submits transcendental problems on the constitution and origin of the universe to the test of the infinitesimal calculus. His judgment in most cases is severe, excessively so at times; for we notice that in certain questions where the difficulty rather consists in picking out the true theory amongst several that explain the facts satisfactorily, the author declares that none of the solutions are to his liking. This happens, for instance, in the mathematical study of the problem of the conservation of solar heat, which is accounted for by at least three theories: that of the radio-activity of some elements of the Sun; that of the gravitational pressure of the ether, which is proportional to the quotient of the solar mass by the square of the diameter, and that of the fall of meteorites upon the solar surface, and perhaps also a combination of the three causes. But it must be pointed out that Poincaré, like several other men of genius, belonged to the metaphysical school of eternal pessimists who distrust the power of the sciences, including that of mathematics, and go so far as to deny these their faculty of representing absolute truth. Poincaré's tendency at arriving at negative results, makes him sometimes look at problems from so arbitrary a point of view that his conclusions lose a great deal of their value. Thus, in the chapter of the problem of the solar heat, on reaching the mathematical analysis of the theory of the meteoric shower, he resolves his equations by eliminating the factor

XUM

representing the distance of the Earth from the Sun, whilst he would have reached quite different conclusions if he had eliminated the factor representing the velocity of our planet. In short, having only the right to affirm the constancy of one product—that of the square of the angular velocity by the cube of the distance—the author implicitly introduces into his calculations (pages 194 and 195 *ibid*) the arbitrary supposition which allows him, later on, to affirm that the Sun cannot have received the necessary amount of cosmic matter to conserve its heat, without the Earth suffering an acceleration sufficiently great to have been detected.

It was Poincaré who wrote in his celebrated "La Science et l'Hypothèse" this disconcerting phrase: "*Les axiomes de la géométrie ne sont que des définitions déguisées*" (the italics are Poincaré's), and this in order to deprive the most beautiful of the pure sciences of its majestic prestige, and to concede belligerence to other so-called geometries. And then he affirms (page 66) that one geometry cannot be truer than another, but only more convenient ("Une géométrie ne peut pas être plus vraie qu'une autre, elle peut seulement être plus *commode*")—again, the italics are Poincaré's. So that if we believe that one side of a triangle is less than the sum of the other two and greater than their difference, or that the sum of the three angles of a triangle is equal to two right angles, it would not be, according to this way of reasoning, because it is absolutely true, but because it would be more convenient to have it so than to believe that the said sum is equal to more than two right angles (geometry of Riemann), or less than two right angles (geometry of Lobatchewsky).

Now, if we measure the angles in millions of triangles, we shall always see that their sum is equal to two right angles, and the same verification can be made with any other conclusion of pure Euclidian geometry, while we fail to verify the conclusions of the pseudo-geometries. Of course, the purpose of those who overlook such facts, is to find a pretext for asserting that it is impossible to find an absolute truth, even in geometry.

We should not forget, by the way, that the school of the impossibilists has been proved wrong on many occasions, and looks like having to sustain a few shocks yet. Pasteur declared that it was impossible to produce by synthesis the substances endowed with the property of polarising light, and yet this synthesis is now a daily affair in laboratories. Auguste Comte was quite certain that man would never know the chemical composition of the stars, and notwithstanding spectrum analysis allows us to analyse a star to-day as easily as we analyse a piece of a mineral.

Louis Favre has rightly said, in his "Histoire Générale des Sciences," that a very interesting "History of Errors" might be written by simply enumerating the supposed impossibilities asserted as such by great men which have turned out in the end to be quite feasible.

There is a "convenience" argument now fashionable amongst many intellectual men, which deals with the rotation of the Earth, and which appears at first sight quite reasonable. It admits the rotation of our planet for the sake of convenience, because there are many facts that support it, and because it is much easier to grant that the Earth revolves around its axis every twenty-four hours than to assume that all the stars revolve around us in one day; but, after all, it is contended, many millions of people, after observing the apparent movement of the Sun, mistakenly affirmed that it was our great luminary which effected its revolution every twenty-four hours; so that this is not a question of pure mathematics, and our belief cannot be called an absolute truth.

To say this, is to overlook the fact that this truth, in common with many others, can be proved mathematically by the application of certain principles of pure mathematics, not to non-exact sciences, but to other purely mathematical principles. For instance, astronomical trigonometry enables us to calculate the distance separating us from Mars, and Kinetics tell us that, in order to revolve round the Earth in twenty-four hours, the red planet would need to possess an orbital velocity of about nine thousand miles a second. Besides, Dynamics tell us that if a body revolves around another, at the mean Martian distance of one hundred and thirty million miles with a velocity of nine thousand miles a second, the central body requires a mass nearly one hundred thousand million times greater than that possessed by the Earth. And since a thing cannot be millions of times greater than itself, we see, by pure mathematical reasoning, that Mars cannot revolve around our planet. And the same reasoning applies to every celestial body except the Moon. For instance, were Alpha Centauri, the nearest star to the solar system, to revolve around us, we should require for our poor little abode a mass many millions of times greater than that calculated by Lord Kelvin for the whole of the visible Universe. So, if we are compelled to recognise that it is not the stars which gravitate around us in twenty-four hours, but that it is our Earth which performs its rotation in one day, we have been forced to accept this conclusion, not only from convenience, but also as an irresistible consequence of a purely mathematical reasoning.

It is not only in such complex matters that mathematics show us their power and their beauty. The simplest problem affords them boundless opportunities. For example, if we ask algebra for two numbers of which the sum shall be four and the product twenty, it gives us as a result two imaginary quantities: the first is two plus the imaginary unit multiplied unto four, and the second is two minus the same imaginary unit multiplied also unto four. And yet, though none of these numbers be real, their algebraic sum is four, and their algebraic product is twenty, as required. It is as if this noble and infallible science, while reminding us gently—by giving us imaginary quantities—that we were talking nonsense in asking for such an unreal thing, wants at the same time to show that even things which are physically impossible to us are not outside her powerful grasp. Do they not give us the weight of a double star as easily as scales give us the weight of a loaf?

It is they, the majestic and poetical mathematics, the charming friends who always tell the truth, who never deceive, flatter or discourage, who have pointed out to Professor Bickerton the secrets of the partial impact in the birth of the new stars, they who gave Adams and Le Verrier news of the existence of Neptune before Galle discovered the most remote of the known planets, they, in short, who have whispered to Sir Joshua Thomson the romance of the electrons, those ultimate particles of matter which perhaps the eye of man will never see!

REVIEWS.

ASTRONOMY.

Catalogue of 9842 Naked-eye Stars for the Epoch 1900.—By T. W. BACKHOUSE, F.R.A.S.

(Sunderland: Hills & Co. Price 10/6.)

This catalogue was drawn up with a view to the preparation of fourteen large star maps on the gnomonic projection, designed for use in meteoric observations. The author, however, has gone far beyond what was strictly necessary for this purpose, and the volume will be useful to many besides meteoric observers. It contains all the stars that can be readily seen with an unaided eye of normal acuteness in the entire heavens from pole to pole; several are also included which, though individually too faint, might be seen in conjunction with near neighbours. They are arranged by constellations in alphabetical order, and this is not a very convenient plan, for the boundaries of the constellations are irregular and differ in different atlases, besides which the

mapping of a single region of the sky may require consultation of several constellations in widely different parts of the book; arrangement by Right Ascension in the usual manner of star catalogues would have been more convenient.

Very full details about the stars' nomenclature and magnitude are given; seven different columns are taken up with the Greek or Roman letters, and the numbers in various catalogues. Then twelve columns contain the estimates of magnitude by different authorities, and the finally adopted magnitude, using the Harvard scale. This is given to the nearest tenth of a magnitude. The positions for 1900 are given to the nearest tenth of a minute of time in R.A., and the nearest minute of Declination. This is near enough for identification, but necessitates reference to some other catalogue if an accurate position is required.

A column of remarks gives notes about variability, and large discordances in the magnitude by some authorities. One obvious use of the catalogue is to rapidly construct a chart of the region round some Nova or Variable, with definitive magnitudes of the stars included. I used it in this way for Nova Geminorum last spring, and found it very convenient. A series of maps is promised containing all the objects in the catalogue. I have not yet seen these, but from the description given I have no doubt that the combined work will be of great service to a wide circle of astronomers, and will do much to stimulate the study of the brighter stars.

A. C. D. CROMMELIN.

Observatoire Royal de Belgique; Annuaire Astronomique pour 1913.—By G. LECOINTE. 516 pages. 6 illustrations. 7-ins. × 5-ins.

(Brussels: Hayez.)

This forms a serviceable handbook of general astronomical information. It has copious extracts from the *Nautical Almanac*, giving positions of Sun, Moon, planets, satellites, stars, eclipses, and so on, also refraction and tide tables, elements of planets, comets, and so on; an essay on the measure and determination of time, others on recent progress in astronomy, the eclipse of last April, variable stars, terrestrial magnetism. There are reproductions of photographs of Comet 1910 I and of the spiral nebula in Canes Venatici. Altogether a convenient work of reference.

A. C. D. C.

BIOGRAPHY.

The Early Naturalists. Their Lives and Work. (1530–1789).—By L. C. MIALL, D.Sc., F.R.S. 396 pages. 9-in. × 6-in.

(Macmillan & Co. Price 10/- net.)

There is always a tendency more or less to neglect the ladder up which one has climbed; but it is surely part of the education of a scientific man to know something of the steps by which his subject has progressed, something of the men, whose labours formed the foundation and indeed built the edifice of his science. There is so much to do at present, however, that there is little time to give to thought even of the future, let alone to the past, and it is difficult for busy people to study in full detail the life of the great workers who have gone before. For biologists, however, something has been done, for Professor Miall has spent his leisure in surveying the lives of the early naturalists; from Otto Brunfels, who was born in 1484, down to Buffon, who died in 1788, and not only has he written an account of their work, but also in most cases given a carefully considered estimate of their character and of the part which they have played in the building up of biology.

Quite as interesting as the discoveries which were made are the records of mistakes, and there are hosts of interesting notes which illuminate the book as one goes along, dealing with such facts as that the early naturalists could only get their training in the medical school, and that many also found the easiest way to earn a living was to practice medicine. Professor Miall has laid all naturalists under a deep obligation.

WILFRED MARK WEBB.

BIOLOGY.

British Plant Galls.—By E. W. SWANTON. 287 pages. 32 plates (16 being coloured), 7¾-in. × 5¼-in.

(Methuen & Co. Price 7/6 net.)

Very much interest is aroused by galls; very little of a popular character has been written upon them: the result is that the ordinary lover of nature knows practically nothing about them, and the naturalist's ideas are more hazy than they otherwise would be.

Mr. Swanton's efforts to bring together what is known about our British galls are worthy of the highest commendation, and the book which is the result will prove exceedingly useful. It may also lead to more attention being paid by amateurs, who often have the most time to give to such matters, to the study of galls. There is a variety about the subject which it is difficult to equal; the growths are vegetable and arise from the stimuli supplied by animals. Sometimes the species of the latter is only to be determined by the appearance of the galls. The "causers," as Mr. Swanton calls them, may belong to many orders of insects, to the mites, to the eel worms, to fungi, and those to whom the continuous study of a single order or group does not appeal will have the satisfaction of jumping first to one and then to another in dealing with galls.

The alternation of generations which is seen in some of the gall causers is a fascinating subject, and we foresee for Mr. Swanton's book a wide circulation.

From a scientific point of view the classified and descriptive catalogue of British Galls is a most valuable addition to works of reference and the basis of classification is a botanical one, as the majority of British naturalists have some knowledge of our native plants. The first catalogue of British plant galls, published in 1872, contained ninety-one galls; in 1898, Mr. Connold described four hundred and twenty-five, and in the present catalogue more than eight hundred are dealt with. Mr. Swanton appeals for criticisms and specimens with a view to making a second edition of the book more perfect. There is an abundance of coloured and other illustrations; the preface written by Sir Jonathan Hutchinson is another point of interest. The position of the so-called "crown galls" is discussed in this, Sir Jonathan being of opinion that they should be divided from the rest and placed with diseases known in England under the name of "canker."

WILFRED MARK WEBB.

CHEMISTRY.

Oil Finding.—By E. H. CUNNINGHAM CRAIG, B.A., F.G.S. With an introduction by SIR BOVERTON REDWOOD, Bart. 195 pages. 13 plates. 18 figures. 9-in. × 5¾-in.

(Edward Arnold. Price 8/6 net.)

This book will appeal to several classes of readers, for the geologist will here find an able description of the structure of the formations in which petroleum deposits occur, while the chemist will be interested in the chapters discussing the origin and mode of formation of petroleum.

The author's descriptions are not merely impersonal outlines, but are also clever criticisms of the different theories that have been brought forward. He himself strongly favours the view that "every hypothesis but that of the origin from terrestrial vegetation fails when tested by an appeal to the facts to be observed at the present day."

The book is well illustrated, and has a preface by Sir Boverton Redwood, in which attention is called to the value of the book as a means of assigning the true value to the flowery estimates of the oil company promoters.

C. A. M.

METAPHYSICS.

Studies in Jacob Boehme.—By A. J. PENNY. 475 pages. 2 plates. 9¼-ins. × 6-ins.

(John M. Watkins. Price 6/- net.)

One of the marked characteristics of the present century, distinguishing it from that immediately preceding, is to be

XUM

found in the attitude of scientific men towards the problems of philosophy. The extraordinarily rapid progress made in physical science during the nineteenth century created a false atmosphere of finality and assurance, resulting in a belief that all the important facts of the universe were known (only details remaining to be filled in), and that the metaphysical system known as materialism had spoken the last word as to the constitution of the Cosmos. Recent discoveries, both in physics and psychology, have shown that this conclusion was too hasty, and the philosophical outlook of scientific men to-day is distinctly transcendental and idealistic. Scientific men, therefore, are more likely to be interested, nowadays, in the philosophy of Jacob Boehme, than would have been the case had their attention been called during the past century to a book like that now under review.

The present volume forms a supplement to a re-issue of Boehme's works now taking place under the general editorship of Mr. C. J. Barker. Mrs. Penny's essays are by no means free from defects. Of course, one cannot expect the same quality of finish in essays reprinted from periodicals as one would have expected had the authoress lived to write a book in exposition of Boehme's system. But apart from this fact, her sparseness of critical powers, her lack of scientific knowledge, and her habit of treating too seriously the utterances of quite unimportant people, lessen her value as an expositor of Boehme. On the other hand, few persons indeed have studied Boehme to the extent that Mrs. Penny did, and she certainly possessed a valuable knack of bringing together passages from Boehme which mutually enlighten each other. When studying a writer so obscure in style as Boehme, every help is valuable, and Mrs. Penny's book may certainly be recommended to sympathetic readers, though they may not invariably agree with her interpretations of Boehme's theories.

Boehme's theory of the seven properties of Nature is particularly interesting, once one becomes used to his quaint terminology, borrowed largely from the alchemists. It permits of both a metaphysical and a physical application. In its former aspect it asserts that all objective existence is the result of desire operating through imagination and will, and thus approximates to the widely-held philosophical doctrine that the so-called objective world is an ideal construction of the perceiving mind. In its physical aspect there is, perhaps, some vague and slight foreshadowing of the theory that matter is generated by the formation of stress centres, or centres of contraction, in the ether.

H. S. REDGROVE.

THE BRITISH ASSOCIATION AT DUNDEE.

THERE is no doubt but that the Dundee Meeting of the British Association was a decided success, and quite apart from the energy, skill and tact of the local organisers there is one outstanding reason why the meeting was a large one.

Dundee is more than two or three hours' journey from London and indeed from other centres of activity in England. The distance and the fact that Dundee is in Scotland suggested to would-be visitors that attendance at the meeting would really mean a change and give them the opportunity of taking a holiday afterwards in the Highlands or elsewhere. There was also little to encourage readers of papers to pay a flying visit, make their communication and return home the same or the next day, as was possible at Leicester, Sheffield and Portsmouth. Then again residents at such places as these have had many opportunities recently of easily attending meetings, but it is forty-five years since the Association last met at Dundee, and twenty or so since it went to Edinburgh. These facts explain, no doubt, the large number of local members who were a feature of the meeting. The same results were obtained by the Royal Agricultural Society so long as it went sufficiently far from London; but when it made Park Royal its headquarters its annual shows were not a success.

The writer chose the West Coast route, went by the London and North-Western Railway to Carlisle, and thence over the Caledonian line to Dundee, missing the Tay bridge, but passing through many lovely stretches of country.

Speaking of the West Coast, it sounds strange when one hears it said that Liverpool is very nearly as far east as Dundee, although they are on different sides of Great Britain. A glance at a map will soon show that this statement is correct.

Turning to the President's address, which by a coincidence was begun on the very day and at the same hour, forty-five years after the last one delivered at Dundee, it may be said that no other communication is so easy to criticise or more difficult to compose. If the speaker talks of his own work he is liable to be, or to be considered, egotistical, and his matter (unless he is one of those scientific men who are out of a groove and is therefore paradoxically not looked up to by his fellows) is likely to be dry and not appreciated by a general audience. The specialist trying to be general is also in difficulties. His scientific hearers are liable to call his address twaddle. As a matter of fact the writer has only heard one presidential address of this kind which really pleased him. This was given to the South-Eastern Congress of Scientific Societies, by Professor Silvanus Thompson. As the Presidents of this Union in many cases occupy the Chair of the British Association it is possible that the latter body may have a treat in store for them.

Professor Schäfer's address at Dundee certainly gave a good idea of what is at present known with regard to the physiological topics on which he touched, and it was generally appreciated by the mass of his audience which was not made up of specialists. In this respect it is certainly to be commended. It was, however, in the opinion of those who know what makes for the success of a discourse, interminably long, and occasionally when Professor Schäfer emphasised the fact that he was descending to his audience's level, as when he suggested that few present understood the working of a microscope, his remarks were not pleasing. One cannot help thinking how very different it would have been if Professor Schäfer could have spoken instead of read his address. We look forward to the time when some President will break away from the traditional method and give an address more after the style of the best of the evening discourses. Two of the latter, as usual, were given at Dundee. The writer was not present at the first on "Radiation"; but this seems to have been above the heads of the audience. Professor Keith's was certainly excellent, though a little inclined to be like a sermon, while it was very difficult in a large hall to see the specimens of skeletons and skulls and casts with which he illustrated his remarks on "The Antiquity of Man."

One discussion which attracted considerable attention was that arranged on "The Origin of Life" by Section D (Zoölogy), partly from its title and partly as it was in a measure dealt with on the subject of the Presidential address. It was useful in that it showed what various opinions are held collectively and individually, though the comment was that no progress had been made. As we outlined in our last number the presidential addresses of the Sections were of more than general interest. That in Section D, by Dr. Chalmers Mitchell, was devoted to the Protection of Animals and Plants in the World generally. Some of the papers in the same section dealt with the matter also, and a good deal was said on the subject at the Conference of Delegates.